DOUBLE BLIND RESEARCH IN PSYCHOTHERAPY

F.L. Wojciechowski

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PROEFSCHRIFT

ter verkrijging van de graad van doctor
in de Sociale Wetenschappen
aan de Katholieke Universiteit te Nijmegen,
op gezag van de Rector Magnificus
prof. dr. J. H. G. I. Giesbers
volgens besluit van het College van Dekanen
in het openbaar te verdedigen op
maandag 17 december 1984,
des namiddags te 4 uur

door

FRANZ LAURENS WOJCIECHOWSKI
geboren te Heerlen

SWETS & ZEITLINGER LISSE
1984

Promotor: Prof. Dr. C. P. F. van der Staak
Co-referent: Dr. C. P. D. R. Schaap

CIP-GEGEVENS KONINKLIJKE BIBLIOTHEEK, DEN HAAG

Wojciechowski, Franz Laurens

Double blind research in psychotherapy / Franz Laurens Wojciechowski. - Lisse : Swets & Zeitlinger. - Ill.
Proefschrift Nijmegen. - Met lit. opg. - Met samenvatting in het Nederlands.
ISBN 90-265-0586-8
SISO 607.2 UDC 615.851
Trefw.: psychotherapie.

Omslag ontwerp H. Veltman

Gedrukt bij Offsetdrukkerij Kanters B.V., Alblasserdam

ISBN 90 265 0586 8

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The research reported in this work was made possible by a grant from the Catholic University of Nijmegen. The preparatory research of the literature was carried out largely in 1979. Experiment I was executed in the winter of 1980/81, and experiment II in early 1982. The bulk of this publication was written in 1983.

Originally it had been my objective to study the so-called nonspecific, therapeugenic, common, or – if you wish – "placebo" factors in psychotherapy. It was my intention to isolate these factors deemed critical to psychotherapeutic effectiveness through a survey of the pertinent literature, and to study a few of the more central ones in a number of experiments. Locating relevant factors in the literature was not so difficult, but devising the experiments proved to be quite problematic: Psychotherapeutic outcome research did not impress me as being very sophisticated and appeared to be quite bias prone, as was testified by the host of contradictory and irreplicable results in the literature. Therefore, in an early stage, I decided to shift my emphasis to psychotherapeutic research methodology, to try to design a research method that might provide less irreplicable results.

In my search for a methodology, one of my vices – the love for good wine – proved to be a great help: I was accustomed to have regular wine tasting evenings with my

brothers during which we did the tasting "blind". In accordance with well established tradition in vinophile circles we tasted the wines without knowing the name or vintage of the wine so that we would not be biased in our judgements by impressive names of famous châteaux or vintages. Experience taught us, as many other vinophiles had discovered before, that – tasted blindly – famous wines may sometimes be disappointing in taste and that less well-known wines may sometimes turn out to be unexpectedly good. The blind tasting of the specific wines that are being evaluated was developed to control for taster bias, so that famous wines might not be overrated and wines with a less impressive background might not be underrated.

By studying the literature concerning outcome research strategies, I learned that double blind research in medicine is a procedure quite similar to the blind tasting of wines: In a double blind study in pharmacology both the patient and the therapist are blind as to which patient receives the supposedly active pharmacon and which patient receives the inert pharmacon (the placebo pill). This way both therapist bias and patient bias are prevented from confounding the results: Improvement in the experimental condition will not be overrated and improvement in the control condition will not be underrated. Moreover, a patient receiving an inert pill and knowing it, might well consider himself to be getting a fake treatment and consequently not improve or even quit treatment altogether. Patient blindness also controls for this potential source of bias. Finally, by keeping the therapist blind he is not only prevented from underrating or overrating improvement selectively, but also prevented from acting differently in the experimental and control conditions, thus confounding the results.

In pharmacological research controlling for therapist bias and patient bias is considered necessary in order to obtain valid and reliable results. I felt that if this holds for pharmacotherapeutic research, then it should

hold a forteriori for psychotherapy, where the amount of patient-therapist interaction and the opportunities for therapist bias and patient bias to occur are so much greater. The present work is the result of this consideration.

CHAPTER 1: INTRODUCTION AND PROBLEM

The field of psychotherapy has recently been characterized as being in a "rather chaotic state of affairs" (Garfield, 1980:vii). The reasons for this statement are obvious: Over the years a bewilderingly large number of different psychotherapeutic approaches have come into existence. The Report of the Research Task Force of the National Institute of Mental Health (1975) cites the existence of over 130 psychotherapeutic modalities, and more recent findings (Corsini, 1981:x) almost double this figure: "As the reader can see by examining the list of psychotherapies following the preface some 250 systems of psychotherapy are noted."!

All these different approaches claim to do the same thing: to be effective in treating the neurotic disorders. Furthermore, "all appear to lay claim to being the most effective type of psychotherapy." (Garfield, 1980:240). Nevertheless, many psychotherapeutic systems have produced little or no research in order to demonstrate their effectiveness, and the research that does exist on quite a number of different modalities, has not been able to show "that one technique is clearly superior to another, even under reasonably controlled conditions" (Strupp, 1978:11).

Whenever a new system of psychotherapy appeared on the scene, there were claims of unique and superior effectiveness. However, research findings after the initial period of enthusiasm always disconfirmed these claims. In its

early years, for instance, behavior therapy claimed and reported 90% improvement rates (Wolpe, 1958). This is impressively more than the two-thirds improvement reported for psychotherapy in general (Garfield & Bergin, 1978). Recent reports, however, have concluded that the effectiveness of behavior therapy does not differ significantly from that of other psychotherapies (Luborsky, Singer & Luborsky, 1975; Shapiro & Shapiro, 1982; Sloane, Staples, Cristol, Yorkston & Whipple, 1975; Smith, Glass & Miller, 1980).

One of the latest therapeutic "fashions", cognitive behavior therapy, shows a similar trend: "Twenty years ago the advent of behavior therapy was heralded by impressive claims of effectiveness, claims that its proponents now admit sometimes outstripped the available research evidence. Now cognitive behavior therapies may be riding a similar wave of initial enthusiasm" (Miller & Berman, 1983:50). In their review of the research evidence they had to conclude that, although cognitive behavior therapies were clearly better than no therapy, there was no firm evidence that these therapies were superior to other psychotherapies. In the same light, one may recall an even more extreme case, the rise and fall of biofeedback. This form of therapy, which was hailed as a kind of cure-all and final answer to mankind's neurotic and stress-related disorders in the late sixties and early seventies, has now faded into the background mainly because of the less than miraculous results.

All in all, recent reviews of psychotherapeutic research converge on the conclusion "that diverse psychotherapies are modestly, but equally, effective" (Shapiro & Shapiro, 1982:581). This has resulted in a search for the "common denominators" of psychotherapy: When all these different approaches produce comparable results, even though they are often based on conflicting theories of personality and personality change and use procedures that can be diametrically opposed to each other, then it may be the common elements rather than the distinctive features which

produce the results. This line of thought has been pursued by investigators like Frank (1974), Garfield (1980), Nawas, Pluk & Wojciechowski (1980), and Shapiro & Morris (1978). Therapeutic improvement has been linked to such "placebo" and "nonspecific" factors as hope, faith, trust, expectancy of improvement, etc.

Nonetheless, the fact remains that the various therapies are only "modestly" effective and that all procedural innovations, modifications and research efforts of the last forty years have not been able to improve upon this sad state of affairs (Garfield, 1981). Put more concretely, we still lack an answer (even a partial answer) to what may be the most important question in psychotherapy: "What treatment, by whom, is most effective for this individual with that specific problem, and under which set of circumstances?" (Paul, 1967:111).

The fact that the research efforts which have been undertaken in the past forty years have led to little more than the realization that we do not have an answer to the aforementioned question (despite all the clinical lore that pretends we do), in addition to the fact that the various psychotherapies appear to be equally, but only modestly effective, has led to diverse reactions. Some clinicians have become totally disenchanted with research and have divorced their practice from any research whatsoever; others have turned their backs on the field of psychotherapy; and still others have become selfconscious practitioners haunted by the shaky base of their profession. Nevertheless, were it not for this research, we would still be harboring omnipotent thoughts about the effectiveness of current psychotherapeutic procedures, and applying the methods we have learned uncritically. Furthermore, it is only from empirical research that we may ever hope to obtain an answer to the above question, so succinctly posed by Paul.

When we turn our attention to the related field of medicine,we find some remarkable similarities to the situation

described above: When the science of medicine gradually emerged from the art of medicine in the late nineteenth century, there was a long period of confusion, uncertainty and disenchantment with the results of medical therapies too. A 1885 editorial in the "Medical Record" described the spirit of the moment as "profound scepticism regarding the efficacy of many of our therapeutic endeavors" (Shrady, 1885:577). And Oliver Wendell Holmes, one of the leading physicians of his day, sadly concluded that "if the whole materia medica as now used, could be sunk to the bottom of the sea, it would be all the better for mankind, — and all the worse for the fishes." (Holmes, 1892:203).

This therapeutic scepticism, often amounting to nihilism, was largely due to the fact that also in medicine the results of clinical trials and controlled studies at first only seemed to refute the claimed unique and superior effectiveness of the treatment under study. In due time however, after the development of new treatments and drugs, as well as the refinement of research strategies, medicine has become what it is now: A field that (despite its obvious shortcomings) can boast of an impressive number of "specific" treatments, that can alter the natural course of a disease significantly.

As was mentioned earlier, until now psychotherapeutic research has known no real breakthroughs, and "despite claims to the contrary, the innovations and modifications have not produced truly remarkable results" (Garfield, 1981: 182). In light of these circumstances it seems worthwhile to investigate how medicine managed to emancipate itself from a similar situation; how research efforts in that field have eventually produced the desired results, and what psychotherapeutic research might learn from this.

In Chapter 2 the state of affairs in medicine before the "scientific revolution" will be described, and the development of research methodology in medicine will be traced. It will be shown that controlled research, culminating in

the development of the so-called double blind design, was the motor behind the therapeutic breakthrough in medicine. In pharmacological double blind research both therapist bias and patient bias are controlled for by using outwardly identical pharmaca and by not informing the patients or the doctors which patient is receiving the experimental pharmacon and which patient is getting the control pharmacon (the placebo pill). This way both patient and doctor expectancy of improvement and related phenomena are prevented from influencing improvement or reported improvement differentially across experimental conditions.

Chapter 3 will be devoted to a historical review of research strategies in psychotherapy and these will be compared to those used in medicine. The evidence presented will show that controlled research in psychotherapy is of comparatively late vintage and that it has not progressed beyond the single blind level. In the single blind design patient bias, but not therapist bias, is controlled for. Double blind research has been proclaimed impossible in psychotherapy.

In the empirical section of this work (Chapters 4 and 5) two experiments will be presented which have the following objectives: (a) to investigate the effects of therapist bias in psychotherapeutic outcome research, and (b) to show that it is possible to adapt the double blind design of pharmacotherapeutic research in such a way that double blind research, and thus controlling for therapist bias, is feasible in psychotherapeutic research too. Chapter 6 will give a summary of this research and discuss its implications.

2.1. PRESCIENTIFIC MEDICINE

Looking back at the practice of early medicine, we are confronted with a bewildering and sometimes shocking picture. Therapies were often bizarre and in the great majority of cases drugs were used that were pharmacologically worthless or even harmful.

In ancient Egypt, for instance, patients "were often treated with such delicacies as lizard's blood, crocodile dung, the teeth of swine, the hoof of an ass, putrid meat and fly specks" (Findley, 1953:1822). The aforementioned therapies can be found in the Papyrus Ebers, written about 1600 BC. Shapiro (1977) concluded that none (!) of the 800 therapies and 700 drugs mentioned in it had any specific value for the condition treated. This is also the case with all the therapies found in the writings of Galenus (Shapiro, 1968) and Hippocrates (Houston, 1938). In words that cannot be misconstrued, Houston informs us that: "One scans the pages of Hippocrates in vain for any treatments of specific value" (Houston, 1938:1417).

In the Middle Ages the situation was essentially the same: The most famous medicines of the time were "unicorn" horn, theriac (consisting of 33 to 67 different substances), mattioli (which could contain up to 230 different substances), mummy powder and bezoar stones. As far as the treat-

ed diseases were concerned, all of these were without any specific pharmacological action (Shapiro, 1959, 1960, 1977). In those days medical practice was heavily influenced by magical thinking. Especially astrology played an important role. Many of the plants that were used for concocting medicines were collected only during certain periods of time, in order to potentiate their action in an astrological way! A famous example of medieval medical practice is the treatment that Pope Bonifacius VIII received from his physician A. de Villanova for his kidney troubles: A golden seal with a picture of a lion on it, that was made when the sun was at its zenith, to be worn around the loins (Clauser & Arnhold, 1960). It is interesting to note that the treatment was a success!

This way of practicing medicine had its dangers too, as may be best examplified by the treatment that King Charles II received from the royal physicians during his last illness: ".. a pint of blood was extracted from his right arm, and a half-pint from his left shoulder, followed by an emetic, two physics, and an enema comprising fifteen substances; then a sneezing powder, more emetics and bleeding, soothing potions, a plaster of pitch and pigeon dung on his feet, potions containing ten different substances, chiefly herbs, finally, 40 drops of extract of human skull, and the application of bezoar stone; after which his majesty died" (MacKinney, cited in VanDyke, 1947:322).

The realization that the large majority of therapies and drugs in use were worthless or even harmful (mummy powder for instance contained quite a lot of arsenic), seeped through slowly and in general only partially. Early physicians often scorned the remedies of others (especially if they came from outside the medical profession), but generally applied their own remedies uncritically. Shapiro (1960) cites the instance of Robert Boyle, who removed a sizable number of remedies of dubious value from the pharmacopaea, at the end of the seventeenth century, only to substitute

them by others of equally dubious value, such as the sole of an old shoe worn by a man "that walked much", which was to be ground into powder and taken for stomach ache!

Once in a great while a really useful drug was discovered and used, but more often than not, because there was no method of distinguishing the drugs which had true actions from those that were wholly inert, these remedies became lost to future generations. Therapeutic fashions changed, and useful as well as worthless old medicines passed into oblivion (Pepper, 1943, 1945).

In the light of the foregoing it is not surprising that the history of medicine has been characterized as the history of the placebo effect, because until quite recently almost all medications were placebos, pharmacologically worthless drugs (Houston, 1938; Pepper, 1945; Shapiro, 1960). "In a word, the medicines used were placebos, something to please the patient. The doctor himself, by words of cheer and comfort sought to please the patient. His medicines were merely symbols to reinforce this purpose" (Houston, 1938:1417). Pharmacologically inert or worthless, the medicines worked only because of the factors involved in the doctor-patient relationship, such as the faith, hope and trust both patient and doctor had in the value of the medications, and the faith the patient had in the doctor, aided by the recuperating forces of the organism, the "vis medicatrix naturae".

2.2. SCIENTIFIC MEDICINE

With the introduction of the methods of natural science into medicine in the eighteenth century, and their gradual acceptance in the nineteenth, the picture slowly began to change. Increased knowledge of anatomy and pathophysiology produced a more realistic insight into the nature of disease, a decline in speculative a priori theories about the causes of disease and a decrease in the fallacious "post

hoc, ergo propter hoc" reasoning, which had characterized medicine until then. The first systematic, albeit crude attempts were made to appraise the value of several remedies (Bull, 1959; Heischkel, 1955).

Because of the immaturity of the early clinical trials and the fact that until well into the nineteenth century few really active medications (such as mercury, quinine, opium, and purgatives) were available, there was only very little progress at first. The general lack of positive results attained by the early clinical trials led to an era of therapeutic scepticism in the second half of the nineteenth century. This lasted until the nineteenthirties (see DuBois, 1938; Forel, 1918; Holmes, 1892; Kelly, 1962; Shrady, 1885). In that period medical research, provided with the many new drugs which had been developed after the enormous development of organic chemistry since the late nineteenth century (Bull, 1959), fruitfully combined the research techniques developed over the past few centuries to give birth to objective clinical research (Hordern, 1968).

Was it still necessary for E.F. DuBois, President of the Association of American Physicians, to lament in his 1938 Presidential Address that there had been ".. a striking lack of studies dealing critically with the great mass of drugs in the great majority of our patients" (DuBois, 1939: 1), and that ".. our hospital formularies are full of worthless drugs and they should be eliminated" (Ibid:5); already in 1953, Findley could conclude with satisfaction that "Any middle aged physician can today look back with awe at the ever increasing number of diseases for which specific remedies or highly palliative procedures have been developed within his memory, and he knows he will live to see many more" (Findley, 1953:1821).

It is time now to take a look at these research methods that "only within the last thirty years have ... been fruitfully combined to produce objective clinical research" (Hordern, 1968:96), that indeed eliminated a host of worth-

less drugs from medical use and established the value and indications of many others.

2.3. CONTROLLED CLINICAL TRIALS

As indicated above, many of the individual features of controlled research are already fairly old. Many examples can be found in the reviews of Bull (1959), Haas, Fink & Härtfelder (1959), Heischkel (1955), and Hordern (1968). In 1721, for instance, Cotton Mather (in Boston) and Jurin (in London) generated statistical comparisons for examining prophylactic methods, and were thus able to show the value of smallpox vaccination clearly. In 1793 Cobbett first used statistics in therapeutical research: Based on the death rates observed in Philadelphia, he demonstrated that Benjamin Rush's method of treating yellow fever by bleeding and purgation was not merely useless, but actually injurious to patients. In the mid-nineteenth century, P. Louis drew attention to the necessity of comparing the outcome of treatments with cases differently treated. And in the same period of time a Medical Society in Vienna was carrying out experiments in which both the testers and the tested were unaware of the medicines used.

Even as early as 1747, Lind had used a controlled trial in which he showed that oranges and lemons could cure scurvy: He deliberately chose a homogeneous group of patients and nursed them in the same environment on a diet that was identical, except for the medical treatment. He used six groups of patients consisting of two patients each, and gave them respectively citrus fruits, cider, elixer of vitriol, vinegar, sea water, or a purgative electuary for six days in a row. After that period only the two patients on citrus fruits had improved significantly.

These experiments were, however, exceptions – islands in a sea of "post hoc, ergo propter hoc" reasoning. Also they usually contained only one or a few of the characteristics

of current controlled research. In the 1930s, as was mentioned above, these features were combined to result in the clinical trial as we know it today: A clearly defined target population, random assignment to different treatment groups (or matching on critical variables), adequate sample sizes, standardized methods of measuring outcome, the use of statistics, and finally, the inclusion of placebos (pharmacologically inert medications) and/or no-treatment control groups.

These last two features have been most influential: They allow the separation of the effects of the passage of time (spontaneous remission), the effects of being in treatment, and the effects of the specific medications. The use of control groups, however, created a new problem that needed to be solved: Should the patient find out in any way whatsoever, that he was receiving an inert medication, he would not consider himself to be in treatment anymore, and the purpose of the placebo control group would be defeated. Patients therefore needed to be "blind" as to what treatment they received. In order to achieve this, placebos were made for each experiment that were identical to the experimental medication in all aspects (size, color, taste, texture, etc.) except the pharmacological content. This constitutes the so-called single blind design, in the development of which Martini (1932) played a leading role. In these experiments the patient is blind as to which treatment condition he is in. He does not know whether he is receiving the experimental or the control medication. The most efficient way to ensure blindness in the patients is to use "identical matching placebos" and, to avoid that the patients start guessing whether they are on placebo or not, not to inform them of the inclusion of a placebo control group until completion of the study, viz. "..the patient should not be aware that any experiment is in progress" (Baker & Thorpe, 1957:591). In this way we prevent patient expectancy of improvement and related phenomena from distributing them-

selves unequally over the treatment conditions. (For a discussion of the obvious ethical implications of controlled research, see Bertschinger, 1959; Brownell & Stunkard, 1982; Dollery, 1979; Foulds, 1958; Loftus & Fries, 1979; Sleisenger, 1958; Wagner, 1967; Wilhelmsen, 1979).

However, it is not only the patient who has expectations and hopes. The same holds true for the therapist/investigator. Tétreault & Bordeleau (1971:45) eloquently observe that "The investigator, as objective as he may believe himself to be, is eminently suggestible because he wishes to verify the hypothesis he has posed. If he has chosen to carry out a particular comparison, he does so in the hope of demonstrating the superiority of one medication over others. In order to better establish his position and reputation as a researcher he may feel obliged to publish. In order to survive in his surroundings, it is equally necessary that he obtains financial support. Even if he does not favor any of the treatments under study, negative results disappoint him and render it more difficult to receive funds from either industry or private or public foundations. The researcher must protect himself against his clinical intuitions, which are greatly influenced by his interests."

The awareness that the physician himself may serve as an uncontrolled variable which may completely negate his clinical observations led to the development of the so-called <u>double blind design</u> in the 1940s (Conferences on Therapy, 1946; Greiner, Gold, Cattell, Travell, et al., 1950; Modell, Gold & Clarke, 1945). This design is essentially the same as the single blind procedure, with the additional feature that also the treating physician is kept in the dark about which patient receives the experimental and which patient the control treatment. This way the double blind design controls for patient as well as therapist bias.

This is not a superfluous enterprise, as has been shown by experiments in which the results of single and double blind studies were compared: Many times an experimental

drug which had been established to be effective in single blind research, was proven to be no more effective than a placebo in double blind studies (see Auerbach, 1967; Greiner et al., 1950; Têtreault & Bordeleau, 1971; Weimer, 1965). Notably, but not exclusively, when subjective responses that cannot be observed directly (such as drowsiness, dizziness, headache, pain in general, etc.) are being studied, the results are prone to physician bias (Beecher, 1952; Chassan, 1967). Not only may the physician unconsciously (or even consciously) overrate improvement in the experimental group; by his manner of communicating with the patients he may inadvertently communicate his prognostic expectations to them and directly influence the improvement the patient reports or even the symptoms themselves. A doctor's enthusiastic, hopeful attitude beneficially affects a host of symptoms and complaints. This is a well-known fact (see Feldman, 1956; Shapiro, 1969; Shapiro & Morris, 1978). Examples of the effects of therapist bias in clinical research are legion (see a.o. Downing & Rickels, 1983; Loranger, Prout & White, 1961; MacAndrew & Rosen, 1964; Uhlenhuth, Canter, Neustadt & Payson, 1959), and even in non-clinical research the phenomenon of experimenter/researcher bias has been reported (Rosenthal, 1966; Rosenthal & Rubin, 1978; Wojciechowski, 1982)!

Therefore it does not come as a surprise that in their careful review of controlled clinical trials, Modell & Houde (1958:2197-2198) conclude that ".. in addition to the use of placebo control, the double-blind control should be used whenever and wherever it is feasible. We conceive of no disadvantages in the application of the double-blind control – only protection against spurious data". But at the same time they also warn that the double blind design in itself is no guarantee for objective research. The double blind is a necessary but not a sufficient condition, ".. its use will not validate poorly designed experiments" (Ibid:1298). It is no substitute for the critical elements

of objective clinical research as outlined on p. 12, but a necessary addition to them.

CHAPTER 3: OUTCOME RESEARCH IN PSYCHOTHERAPY: THE SEARCH FOR A METHODOLOGY

3.1. THE EARLY YEARS

Psychotherapy emerged as a distinct therapeutic approach in the late nineteenth century. It took hold in a period that was characterized by disenchantment with the results of physiological therapies as well as a renewed recognition of psychological factors, e.g. suggestion, in the causation and treatment of illness. In those days psychotherapy was more or less synonymous with therapeutical hypnosis ("suggestive therapeutics"). It is hard to find any outcome data in the literature of the day. One of the few who did produce some evidence of the effects of his hypnotherapeutic endeavors was Bernheim (1899). He reports the therapies given to individual patients and the respective results. In modern methodological terminology we may characterize these data as retrospective anecdotal case reports.

Around the turn of the century new approaches to psychotherapy appeared on the scene, psychoanalysis being the most prominent and influential one. When we examine the index of Freud's Collected Writings, we do not come across such subject headings as "research", "outcome", "effect (-iveness)", etc. In a sense this is characteristic of psychoanalysis in general. Especially in its early years, psychoanalysis was characterized by a relative lack of outcome research endeavors. One of the notable exceptions is the

report brought out by the Berlin Psychoanalytic Institute in 1930 (see Bergin & Lambert, 1978:141 ff). In order to illustrate the state of the art at that time, we will focus on this report a little longer. The report contains statistics on the treatments given by the Berlin Psychoanalytic Institute from 1920 until 1930. During those ten years there had been 1955 consultations leading to the commencement of 721 analyses. When the report was written, 363 of the 721 patients had completed treatment, 241 had dropped out, and 117 were still in treatment. Of the 363 who had completed treatment, 111 were classified as cured, 89 as much improved, 116 as improved, and 47 as uncured.

This type of research, retrospective clinical treatment reports which resulted in enumerations of the number of patients cured, (much) improved, or uncured, remained the standard type of research done in psychotherapy well into the 1950s, just like it had been in medical research until the advent of the single blind design in the 1930s. An important reason for this prolonged lack of methodological sophistication was that the clinical orientation that psychiatrists had, was not very conducive to psychotherapeutic research, and they formed the bulk of professionals in psychotherapy in the early years. When psychologists, who had a more solid background in research methodology, stepped into the field of psychotherapy in the 1940s, the dearth of research evidence began to be felt. Snyder remarked in the Annual Review of Psychology of 1950 that: "A very noteworthy trend is increasing awareness of the need for research in psychotherapy. This is most characteristic of psychologists but is appearing also in the writings of psychiatrists." (Snyder, 1950:223).

In 1949, Snyder had initiated a research program at the Pennsylvania State University himself and had many colleagues working under his leadership (Ford, 1959; Snyder, 1957). A positive distinguishing feature of their research project was that, in contrast to the standard of practice

until then , they addressed themselves to the thorny issue of research design before embarking on the psychotherapeutic venture. Regretfully, the outcome of their methodological deliberations was not so positive: "The most rigorous research designs involve experimentation. However, the nature of the treatment setting, with the therapist's obligation to the client, and the client's concern with immediate and pressing problems rather than in advancing science, initially convinced us it was impractical if not impossible to utilize formal experimental designs to study therapy in the clinic setting. Consequently, the more readily applicable historical-descriptive research designs were used. In such studies, one attempts to describe the therapy situation as it typically occurs without any attempt at manipulating variables within the situation" (Ford, 1959:55). This means that their research remained at the old level of uncontrolled, descriptive clinical treatment reports. It is clear that such research cannot produce anything more than suggestive evidence, hypotheses which need to be tested by more rigorous designs. Later in the fifties Snyder et al. realized these consequences of their approach, and reversed their stance on the desirability and feasibility of controlled research in psychotherapy (see Ford, 1959:56). However, this insight has bearing on a later period of time.

3.2. THE EYSENCK ARGUMENT

At this point it is important to realize that, when Eysenck launched his attack on psychotherapy in 1952, this was the quality of research evidence he had to draw upon. We refer here to Eysenck's article which began with the rather innocent title "The effects of psychotherapy: An evaluation" and ended with the sweeping conclusion that "The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder" (Eysenck, 1952:323). This conclusion shook the psychotherapeutic community to its

core, and led to a polemical discussion about the value and effectiveness of psychotherapy that is still raging today. This, in addition to the fact that his conclusions gave new impetus to more adequate outcome research (because as Thorne, 1952:40, justly stated ".. the burden of proof rests upon those who make therapeutic claims"), justifies a closer look at the Eysenck argument.

Eysenck clearly realized that in order to evaluate the effectiveness of psychotherapy, he needed data from a control group of untreated patients. This way he could compare the effects of therapy with the rate of spontaneous remission. However, data of this kind were non-existent at the time: Up until then no controlled outcome research had been done in the area of psychotherapy. He did find an alternative in the literature, however. In 1938 Landis had computed the percentage of neurotic patients who were discharged annually as recovered from New York state hospitals for the years 1925 to 1934 and for the U.S.A. as a whole for the years 1926 to 1933. These were respectively 70% and 68%. The "consolidated amelioration rate" for the period 1917 to 1934 for New York state hospitals was 72%. This led him to conclude that more than two-thirds of severe neurotics, who were mainly receiving only custodial care and little or no formal psychotherapy, recovered or improved to a considerable extent. In 1946 Denker studied the results of 500 consecutive cases of disability claims due to neurosis, which had been treated by general practitioners (with sedatives, tonics, suggestion, and reassurance – but no systematic psychotherapy). He found that 45% recovered within one year and an additional 27% in the second year, totalling 72%. This is a very close approximation of the Landis data. Consequently, Eysenck concluded that in the absence of formal psychotherapy, about two-thirds of the neurotic patients improved considerably within two years.

Summarizing the statistics produced by the five clinical treatment reports of the effectiveness of psychoanalysis,

with a total of 760 patients, Eysenck found that on the average, 44% of the patients were reported to be cured or much improved after therapy! The nineteen studies of eclectic psychotherapy reviewed by him, totalling 7,293 patients, averaged 64% cured or much improved. Admitting that there are obvious shortcomings to any actuarial comparison such as this one, he nevertheless felt secure enough to conclude that the psychotherapeutic claims of effectiveness had remained unproven.

As stated before, this article led to a hot debate about the validity of the data used. The participants debated such topics as the overstrictness in which Eysenck applied the criteria of "recovered" or "much improved", and the leniency with which he applied the "spontaneous recovery" criterion; and they got entangled in claims and counterclaims as well (De Charms, Levy & Wertheimer, 1954; Erwin, 1980; Eysenck, 1965; Luborsky, 1954; Rachman & Wilson, 1980; Rosenzweig, 1954). Most importantly, however, the article led to a more systematic approach to psychotherapeutic research, a result that is in line with Eysenck's emphasis on the fact that the shortcomings of the data "highlight the necessity of properly planned and executed experimental studies in this important field" (Ibid:323). In this respect Eysenck was not alone, even though his argument has become the most famous one. A similar concern with psychotherapy effects and/or more adequate outcome research can be found in about the same time in Appel, Lhamon, Myers & Harvey (1953), Edwards & Cronbach (1952), and Thorne (1952) – an indication that the spirit of the time was changing, and that the concern for methodology in psychotherapeutic research was no longer limited to only a few isolated individuals.

3.3. THE POST-EYSENCK PERIOD

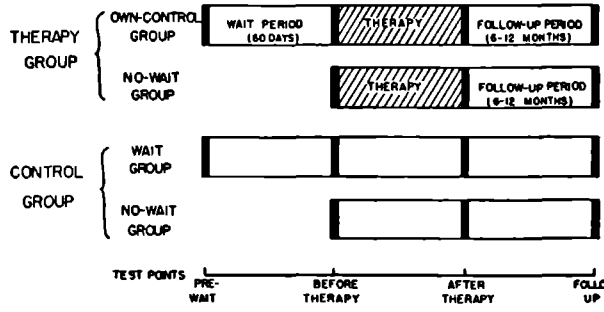
It was especially the problem of adequate no-treatment control groups that became a concern after the Eysenck attack:

Psychotherapy needed to prove that it was better than the mere passage of time. (Incidentally, Eysenck did not really prove that psychotherapy had failed to establish that it was better than spontaneous remission: His "spontaneous remission" data are based on patients receiving what may be called minimal or "nonspecific" treatment. Consequently his paper only supports the hypothesis that the effects of psychotherapy are not greater than those of nonspecific treatment.)

Reviewing psychotherapeutic research, Rosenthal & Frank conclude in their 1956 hallmark article "Psychotherapy and the placeboeffect" that up until then ".. the study which has paid the closest attention to the question of controls in psychotherapy is that of Rogers and his colleagues." (Rosenthal & Frank, 1956:198). This research is described in Rogers & Dymond (1954), a study considered to be a model for adequate psychotherapeutic research at that time. In order to study the efficacy of client-centered therapy, they had utilized the design shown in table 3.1.

From this figure we can see that they employed a therapy group as well as a control group. Part of the therapy group was set apart as an "own-control group" in which there was a sixty day "wait period", preceded and followed by the administration of the research tests, before therapy began. This subdivision of groups also took place in the control group which consisted of a "wait" and a "no-wait" group.

Table 3.1.: General design of the Rogers & Dymond study+



+From Rogers & Dymond, 1954:38. Copyright 1954 by the University of Chicago Press. Reprinted by permission.

The rationale of the design was that ".. through the own-control group, we can control for personality factors and motivation for therapy. This is accomplished by comparing changes made during the wait and the therapy periods. The no-therapy control group provides a more precise control for the passage of time and the effects of repeated administration of tests. If change occurs in the therapy and follow-up period which is greater than the change in the waiting period of the own-control, or in the equivalent period of the control group, we would have strong evidence that therapy produces change which is not accounted for on other grounds." (Ibid:38-39).

A closer look at the design shows that there are some serious flaws in this research: The "equivalent" (no-therapy) control group did not consist of a comparable group of patients drawn at random from the same population as the treatment group (or matched on relevant variables), but was a "normal control" consisting of persons who had not sought psychotherapy. ("The subjects in the equivalent-control group were selected from those persons who volunteered to serve as subjects for a 'research on personality'", Ibid: 45.) Bordin (1974:48) rightfully criticizes this procedure, because ".. it is doubtful that this sample, in no way comparable, could serve any purpose, even that of testing the stability of the criterion measurements, because this characteristic may vary between normal and pathological samples." Even the "own-control" group in the experimental group was biased in its composition: "A client was placed in the own-control group only if it seemed that waiting was not likely to cause him serious discomfort or harm" (Ibid:46). This means that there was a distinct tendency to place the clients who were greatly in need of help in the no-wait therapy group.

The comments above clearly show the methodological immaturity of this research program, which, at the time was considered to be the most sophisticated one available.

It is clear that an adequate no-treatment control group should consist of other patients and not of "normal controls", and that the division into treatment and no-treatment groups should be done at random or by matching on relevant variables.

In due time research of this type developed (see Endicott, 1962:42), but even after the inclusion of an adequate no-treatment group, important problems still remained. Rosenthal & Frank (1956) were the first to draw attention to the so-called placebo effect as a variable to be controlled for in psychotherapeutic research: A no-treatment control can only establish that psychotherapy is better than no treatment at all. It cannot answer the question whether the same effect might not have been obtained by a pseudo-therapy; whether nonspecific, placebo factors such as expectancy of improvement and participating in a treatment per se would not have been sufficient to produce the same results. They conclude therefore, that "To show that a specific form of therapy based on a theory of psychotherapy and neurosis produces results not attributable to the nonspecific placebo effect it is not sufficient to compare its results with changes in patients receiving no treatment. The only adequate control would be another form of therapy in which patients had equal faith, so that the placebo effect operated equally in both, but which would not be expected by the theory being studied to produce the same effects." (Rosenthal & Frank, 1956:300).

This lead was not taken up immediately, however. The disappointing results of the clinical treatment reports with their heterogeneous patient groups and concomitant problems in defining outcome criteria, that still made up the bulk of psychotherapeutic outcome research in the early fifties, had led ".. to general disillusionment in such studies" (Luborsky, 1959:336). At first the result was a "flight from outcome into process" (Hoch & Zubin, 1964): In their disenchantment with the (lack of) results from the crude outcome studies, researchers decided that adequate outcome re-

search was not feasible yet and that, for the time being, they had to satisfy themselves with process research, trying to delineate what actually happens in psychotherapy more clearly. This trend towards process research is clearly reflected in the papers presented in a series of three conferences on Research in Psychotherapy in Washington D.C. (1958), Chapel Hill, N.C. (1961), and Chicago (1966) (see Malan, 1973).

3.4. SINGLE BLIND RESEARCH

In the 1960s the outcome problem and the issue of devising adequate controls was given more consideration again, notably by proponents of the emerging behavior therapy approach. For it is ultimately of no use to know which process variables are important in psychotherapy, if we do not even know if psychotherapy does any good to the patient.

The study that provided a breakthrough of more sophisticated outcome research was the one carried out by Paul (1966). He compared the effects of systematic desensitization, insight-oriented psychotherapy, an attention-placebo treatment, and finally no-treatment on undergraduate students with public speaking anxiety. The 96 subjects who participated in the study were undergraduate students, who had enrolled in a required public speaking course. They had scored high on performance anxiety scales and had accepted the offer for free treatment of their anxiety. Participants were randomly assigned to one of four groups. Therapy was given by five experienced therapists of Rogerian or neo-Freudian persuasion, who had also been trained in systematic desensitization and the attention-placebo treatment. Each therapist used all three treatments. Therapy consisted of five sessions of individual therapy. The standardized outcome measures that were used included performance anxiety scales, observer ratings, therapist ratings, and client self report ratings, etc.

The results: The no-treatment group did not improve significantly on any of the outcome measures, while all three treated groups improved significantly, and significantly more than the no-treatment group. Also, the systematic desensitization group improved significantly more than the two other treated groups on most outcome measures, while the insight-oriented therapy group did not differ significantly from the attention-placebo group on any outcome measure.

Compared to what was up until then standard practice in psychotherapeutic research, the favorable features of this study are apparent: Standardized measures of outcome were used, there was a homogeneous target sample, the different treatments were of equal duration, assignment to conditions was done at random, and in addition to a no-treatment control group there was also an attention-placebo control group. Furthermore, the design was single blind, because the subjects were not informed about the nature of the treatment group they were in (more specifically: they did not know whether they received a "real" treatment or a control treatment, not even that any control treatment was included in the study).

However, a number of shortcomings have been noted too: Sloane et al. (1975), for instance, pointed out that the subjects were not actual patients but volunteers who had been solicited for research purposes. Nor had they sought any treatment for their speech performance anxiety. In other words, it is questionable whether these results hold equally well for "real" patients with "real" problems.

Another flaw in the study concerns the attention-placebo control group. In the Paul study this consisted of the following: The therapist first explained the nature of the subject's problem, established rapport, and then explained the (bogus) rationale and course of treatment. As part of the rationale subjects were told that they had a low level of tolerance for stress and that their specific anxiety was similar to the way they reacted in any stressful situation,

and that their anxiety could be overcome by practising a very stressful task while under the influence of a "fast acting tranquilizer" (actually an inert placebo pill). This way they would gradually develop a tolerance for stress. They were told that the same task had been used for the stress tolerance training of astronauts. Actually the task involved doing a rather boring tracking and discrimination task at a monitor: identifying "disaster" signals on a recorded tape and making discriminations that (so subjects were told) were typically experienced as very stressful.

A number of researchers have questioned the validity of this procedure as well as that of similar placebo control groups (Borkovec & Nau, 1972; Jacobson & Baucom, 1977; Kazdin, 1979; Kazdin & Wilcoxon, 1976; Loney & Milich, 1978): It is possible that the experimental treatment and the attention-placebo control procedure differ in credibility – in the degree in which they inspire expectancy of improvement in the patient, and this may account for differences in observed effectiveness. (That patient expectancy of improvement is an important contributing factor to psychotherapeutic outcome has been amply documented by Goldstein, 1962, among others.) Empirical research has indeed established that control procedures such as Paul's are generally rated as less credible than the experimental treatments which they are compared to (see Borkovec & Nau, 1972). Therefore, control therapies should be designed in such a way that they are just as credible as the experimental treatments. This equicredibility cannot be assumed but must be assessed.

But even then a problem remains: Kazdin & Wilcoxon (1976) correctly noted that if only credibility is controlled for in an attention-placebo treatment, then, as long as equicredibility has been established, virtually any procedure may be used to this purpose. These control procedures may differ vastly from the experimental treatment, and ".. the greater the procedural divergence, the greater the likelihood that some (nontreatment-relevant) divergence in group

procedures contributes to differences in outcome" (Kazdin & Wilcoxon, 1976:741). This testifies to the desirability of devising control procedures that are not only as credible as the experimental condition, but that are also as similar as possible to the experimental treatment as far as non-critical elements are concerned (e.g. equal duration; equal amount of therapist-patient contact; if home practice is required in the experimental treatment, it should also be required in the control treatment; etc.).

When we compare how psychotherapeutic research methodology has progressed up until this point to the development of research designs in medical pharmacotherapeutic research, we see that psychotherapeutic research has had a comparatively hard time in creating adequate single blind conditions. In pharmacotherapeutic research it was much easier to accomplish this: As long as outwardly indistinguishable pills are used, blindness of the patient is easily ensured. And also due to this indistinguisheability, treatment equicredibility and procedural sameness are automatically ensured. In psychotherapy elaborate and often ingenious manipulations have to be devised to accomplish the same purpose. Furthermore, controlled research in psychotherapy is complicated by the fact that in psychotherapy both the specific and the nonspecific (placebo) variables are psychological in nature, while in pharmacotherapeutic research the specific variables are pharmacological in nature and the nonspecific, placebo variables are psychological in nature. This has caused conceptual as well as practical problems (Wojciechowski, 1981).

3.5. DOUBLE BLIND RESEARCH?

The next phase in pharmacotherapeutic research was the development of the double blind design in order to control for therapist bias as well. What has psychotherapeutic research accomplished in this respect? In the previous chapter the development of research strategies in medicine,

especially in pharmacotherapy, was discussed. After a comparison of the single and double blind designs as applied in pharmacotherapeutic research, it was concluded that the double blind design is the preferable procedure, especially when subjective symptoms are being studied.

Psychotherapy deals almost exclusively with subjective symptoms (anxiety, depression, pain, etc.). Controlled studies in psychotherapy, however, are scarce and essentially single blind in nature. The few who have addressed the question of double blind design in psychotherapeutic research agree in proclaiming its impossibility: In the first published book on the placebo phenomenon, Kissel & Barrucand (1964: 192) already concluded "la technique simple aveugle" to be "la seule possible" in psychotherapeutic research. And the only other book on the placebo effect which also addresses itself to research issues agrees that "the method is essentially single blind" and that a "double blind method for evaluating psychotherapy is hardly conceivable" (Jospe, 1978; 192).

Recent examples of individual researchers voicing the same opinion are DiMascio, Klerman, Weissman, Prusoff, Neu & Moore (1979:191): ".. psychotherapy cannot be double-blind designed", and De Jonghe (1980:678): ".. de methode kan niet 'dubbel blind' zijn". In our opinion this conclusion is premature and based on equating one particular form of double blind research, which has become rightfully the standard procedure in pharmacotherapeutic research, with double blind research per se.

To illustrate this, let us take a look at the essential features of the double blind design, and how these are implemented in pharmacotherapeutic research: Here a typical double blind study involves one or more pharmaca plus a placebo pill, all indistinguishable from each other as far as outward characteristics (e.g. color, size, taste, and texture) are concerned. The treating physician does not know when he is prescribing the "real thing" and when he is

giving the placebo.

It is obvious that such a procedure is indeed "hardly conceivable" for psychotherapeutic research: When, for instance, systematic desensitization (SD) is compared with a pseudo desensitization "placebo" control procedure, both are clearly distinct in procedure and easily distinguishable from each other by the therapist. He knows when he is giving SD and when he is giving the control treatment. And in this sense, indeed, double blind research is impossible in psychotherapy.

But there is more to it than just that: Making the experimental treatment outwardly indistinguishable from the control treatment is only one way of producing the critical feature of the double blind design: the blindness of the therapist as to what constitutes the experimental treatment and what the control treatment. In pharmacotherapeutic research, using outwardly indistinguishable matching placebos is the most parsimonious way of creating therapist blindness. However, there are other conceivable methods of obtaining the same result. And there is no reason to assume beforehand that these other ways of solving the problem should be just as impossible in psychotherapeutic research as identical matching placebos are.

In general, every method that successfully manipulates the therapist's attitude in such a way that he perceives the control treatment as being a bonafide treatment with the same standing as the experimental treatment condition effectively produces a double blind design. This can be done by devising a control treatment for the particular treatment under study, that is credible not only to the patient, but also to the therapist, and to present this treatment as a bonafide therapy to the therapists. This way the therapists will think that they are giving a "real" treatment in the experimental treatment condition as well as in the control treatment condition. The two experiments described in the following chapters are applications of this principle.

The first objective of this experiment is to demonstrate the feasibility of double blind research in psychotherapy. The second objective is to investigate the influence of therapist bias on outcome measures.

It was decided to choose a target symptom that is common enough to obtain a reasonable amount of patients for inclusion in the study, and for which a relatively straightforward, easy to apply therapy with accepted effectiveness exists. These requirements were met by tension headache as the target complaint and relaxation therapy as the treatment modality: Chronic recurring headache is probably mankind's most common complaint with an incidence of about 40% (Ziegler, Hassanein & Couch, 1977). The vast majority of these headaches are diagnosed as tension headache, migraine or mixed migraine-tension headache. Tension headache is generally bilaterally localized, with a gradual onset, and is characterized by a mostly dull, aching pain. Migraine is generally unilaterally localized, occurs paroxysmally, is characterized by a throbbing and/or pulsating pain and is often accompanied by such features as nausea and vomiting. Tension headache is the more common headache type, and psychological factors are more clearly implicated in its pathogenesis (Lance, 1978; Wojciechowski, 1977, 1978). Relaxation training as a therapy for tension headache has been the subject of a large number of outcome studies since the early

1970s in which the beneficial effects it has on tension headache have been established (see a.o. the reviews of Beatty & Haynes, 1979, and Blanchard, Andrasik, Ahles, Teders & O'Keefe, 1980).

Taking only one target complaint (tension headache) was done for reasons of homogeneity of the sample; a consideration which also led to the decision to limit the participants to adult females within the age range of about 18-45. Females were chosen, because therapies were to be given predominantly during office hours, when females are more frequently available than males. Furthermore, tension headache complaints (as well as migraine) are generally more frequent in a female population (Wojciechowski, 1977, 1978).

4.1. METHOD

4.1.1. EXPERIMENTAL DESIGN

The double blind design was created as follows: The experimenter devised a "placebo" control treatment for which there exists no rationale as far as the application of it as a therapy for tension headache is concerned. This therapy (concentration therapy) contained all the theoretically noncritical elements of relaxation therapy (same duration, daily home practices, etc.), but not the theoretically critical (muscle) relaxation instructions. (For more information about this treatment see par. 4.1.4.3.3.) This therapy was presented to the therapists as a therapy which has the same standing and effectiveness as relaxation therapy. This information was contained in a written rationale which was given to the therapists as background information before they were trained in the two treatment modalities. The credibility and logical consistency of the theoretical rationale (see par. 4.1.4.2., and Appendix A for the complete text) was first tested by presenting it to several naive colleagues and graduate students in psychology who had a back-

ground similar to that of the therapists-to-be.

In the traditional single blind design the therapist knows that in the control treatment condition the patient does not receive the therapy that is indicated for his complaint. This holds for condition CR in our experiment: The patient receives therapy C (concentration), but therapy R (relaxation) is indicated; the patient does not know this, but the therapist does. In the case of the experimental treatment condition the therapist knows that the patient receives the treatment that is indicated. This holds for condition RR in our experiment: The patient receives therapy R (relaxation) and R is also indicated; both the patient and the therapist instructions are in accordance with this.

In order to produce a double blind design we manipulated the attitude of the therapist in such a way that in addition to the RR condition, a CC condition was set up as well: The patient receives therapy C and the therapist also thinks that therapy C is indicated. For creating a double blind design, and to control for therapist bias, this manipulation is sufficient because the therapist has equal bias towards both experimental and control treatment. However, to investigate the influence of therapist bias the manipulation is clearly not sufficient because therapist bias is not differentially distributed over the two conditions: Therapist bias needs to be manipulated in such a way that both therapies (relaxation as well as concentration) are given under positive as well as negative therapist bias. This way we obtain the following four conditions: the CC and RR conditions produced by the standard double blind design, plus the CR and RC conditions (RC being the condition in which the therapist thinks that the patient needs therapy C, but gets therapy R; and CR being the condition in which the therapist thinks that the patient needs therapy R but gets therapy C instead – which is the standard control procedure in single blind designs (see table 4.1.). Now both the experimental treatment (relaxation) and the control treatment (concen-

tration) are given under low as well as high therapist expectancy of improvement, in other words, under negative therapist bias (RC and CR) as well as positive therapist bias (RR and CC).

Table 4.1.: The four treatment conditions

		"Indicated" Treatment	
		R	C
Treatment Given	R	RR	RC
	C	CR	CC

R= relaxation therapy

C= concentration therapy

N.B.: A waiting-list control group ("W") to study spontaneous remission, the effects of the passage of time, was also included.

The induction of the four treatment conditions in this experiment was accomplished through the information contained in the written rationale given to the therapists. A concise version will be presented here (A more extensive representation is contained in the Therapist Instructions section): Therapists were informed that research in the U.S.A. had established that in the treatment of tension headache, concentration therapy worked better for some personality types and relaxation therapy better for others. However, the design that had been used did not allow a definitive conclusion: A factorial design had not been used. In order to conclusively prove that therapy R worked better for some personality types and therapy C better for others,

in other words, to show that the assignment procedure increased therapy effectiveness, one half of the patients who need therapy R should be given therapy R and the other half should be given therapy C. The same procedure should be followed with regard to therapy C. Therapists were informed that this experiment was a controlled replication study of the U.S. research in which this procedure was followed. In reality, however, assignment to conditions was done at random, and not on the basis of personality type.

4.1.2. SUBJECTS

4.1.2.1. Patients[1]

Patients were 68 adult female tension headache (or mixed migraine-tension headache) sufferers, solicited through advertisements in several local newspapers. Their mean age was 32.6 years (sd=8.2; range 18-45) and the mean duration of headache symptoms 10.3[2] years (sd=6.3; range 1-20).

4.1.2.2. Selection of patients

Advertisements soliciting female tension headache sufferers between age 18-45 to participate in a tension headache treatment project were placed in local newspapers. Respondents (N=97) were invited to a diagnostic interview with the experimenter. Patients were also required to obtain a written statement from their family doctor in which he specified that there was no evidence of organic pathology related to the headache, and that he consented the patient participating in the treatment.

Patients diagnosed as having tension headache or mixed mi-

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1) In this research I preferred the designation "patients" to "clients" primarily because participants identified themselves this way.
2) Actually this figure is somewhat low, because headache duration of more than 20 years was always scored as 20.

graine-tension headache (for diagnostic criteria, see Philips, 1976), with a history of headache activity of at least one year, were accepted for treatment. Patients with other types of headache, and patients who were either overtly psychotic or on antipsychotic medication were excluded from the study. This narrowed the number of participants down to 83. Fifteen of the 83 patients dropped out during the four week baseline period [3], before random assignment to one of the five experimental conditions had taken place. Therefore, the final sample consisted of 68 patients.

4.1.2.3. Therapists

Six graduate students of psychology were recruited as therapists through advertisements on the bulletin boards of the Department of Psychology. They were paid the regular assistantship fee for their participation. Six of the 13 applicants were selected in such a way that there were an equal number of male and female therapists and that they were of approximately the same age (3 males, 3 females; mean age 26, range 24-28). The experimenter provided one week of intensive training in the relevant treatment modalities for each of the therapists.

4.1.3. INSTRUMENTS

4.1.3.1. Pre-treatment assessment

During the four week baseline period all patients filled out the following battery of psychological tests and questionnaires:

3) The minimum baseline period was four weeks, and the maximum seven weeks. The reason for this was, that it took the experimenter almost three weeks to see all of the applicants for the initial interview, after which time each patient started filling out the headache diary cards. When all patients had completed at least four weeks of baseline period, the treatment started. For computation of baseline values of the headache, the last four weeks of baseline of each patient were taken.

a. The Dutch version of the MMPI (Nuttin & Beuten, 1963);
b. The NPV (Nederlandse Persoonlijkheids Vragenlijst) (Luteyn, Starren & van Dijk, 1975);
c. The NSC (Nawas Sentence Completion) (see Appendices B and C);
d. The BV (Biografische Vragenlijst); A translation of the Lazarus' Life History Questionnaire adapted for the purpose of the present study (see Appendix D).

Patients as well as therapists were told that this battery was included so that patients could be assigned to one of the two therapies on the basis of the test results. Actually the MMPI and NPV were only included to convey that idea to patients and therapists, and to check if randomization had taken place on these parameters. The NSC was included for use by the therapists in the first half of each therapy session (see par. 4.1.4.3.1.: Verbal Therapy). The BV was included to obtain information about the patient's background (age, socioeconomic status, etc.). Items to assess the nature and duration of the headache complaint and pre-treatment expectation of therapy outcome were added to the standard form, while items incidental to our purposes were deleted to reduce the length of the questionnaire.

4.1.3.2. Headache diary

After the initial interview patients began headache diaries. They were asked to record whether they had a headache or not; the hours at which the headache started and ended; and, the average headache intensity (on a 12-point scale, ranging from 1=very slight, to 12=excruciating) at the end of each day on a headache diary card (for a copy see Appendix E). Patients in the four treatment conditions were instructed to continue this symptom monitoring until the completion of the study at the follow-up session. The patients on the waiting list continued the headache diaries until the four treatment groups had completed treatment. At that moment

they entered treatment themselves.

A number of different headache activity parameters can be computed from the headache diary:

a. The number of days per week in which there was headache activity (0 - max. 7);
b. Total headache duration per week (in hours) (0 - max. 168);
c. Average weekly headache intensity (0 - max. 12);
d. Medication intake. This consisted of the total number of headache medications taken per week;
e. A weekly headache index (0 - max. 2016). This measure is obtained by multiplying the intensity and duration scores per day and adding these to arrive at a weekly total. This headache measure is considered to be the most sensitive one (Blanchard, Theobald, Williamson, Silver & Brown, 1978), and will therefore be used in this research.

4.1.3.3. Patient Evaluation Form

At the follow-up session, six weeks after completion of therapy, each patient filled out a Patient Evaluation Form (for a copy see Appendix F). This contained questions about the credibility of the treatment received, the results obtained, whether they perceived the length of treatment as adequate or not, how often they had done the exercises at home, and how well they went.

4.1.3.4. Therapist Evaluation Form

Within a week after completion of the treatment of his/her last patient, each therapist filled out a Therapist Evaluation Form (a copy is included in Appendix G). This form was included to obtain information about the attitude of the therapist with respect to the different therapy conditions (did it matter to him/her which condition the patient was in?); his/her opinion about the experimental design

(an indirect way to check whether the double blind design had been realized), and a global rating by the therapist of the patients he/she considered to have improved most and least (to see whether this systematically varied over the various experimental conditions).

4.1.4. PROCEDURES

4.1.4.1. Patient instructions

During the initial interview with the author, patients were also given information about the purpose and procedures of the treatment project. Each patient was told that a number of nonpharmacological, psychological treatment modalities are available for the treatment of tension headache, and that in this project the two most promising ones would be used.

Patients were first given a "headache rationale" in which stress and tension were related to their headache. We also pointed out that when stress and tension have been linked to a resulting headache a number of times, the body acquires, as it were, the faulty habit of reacting to a period of stress and tension with a headache. The "weak link" theory was used to explain why some people get headaches because of stress and others a different symptom under the same circumstances; that a "new tubes" solution to this weak link problem is not possible, that we cannot replace the weak link organ by a stronger one. The alternative, preventing the weak link from overreacting to stress is possible, however: We can teach the body to react differently to stress and tension. That two methods exist for retraining these reactions:Autogenic Training (AT) and Yoga. However, these two methods are not specifically devised to combat tension headache, and consequently contain a number of exercises that are superfluous to tension headache therapy. That two methods based upon AT and Yoga, that do away with these un-

necessary exercises have been developed. One method stresses the meditation-concentration part of the two earlier methods (concentration therapy) and the other method the more peripherically oriented muscle relaxation exercises (relaxation therapy).

Patients were then told that research had produced convincing evidence that both methods were not equally effective for all tension headache sufferers; that some people benefited more by relaxation therapy and others more by concentration therapy. They were also told that the personality characteristics of these two groups differed; that it is possible to decide on the basis of knowledge of these personality characteristics who would benefit most by which therapy; that our treatment project included a battery of tests to assess these personality characteristics; and that on the basis of the test results, each patient would be assigned to one therapy or the other.

Information about the procedure contained the following: The therapy will be individual. The duration is 8 weekly sessions of one hour each. The majority of patients will enter treatment four weeks after the initial interview, after we have obtained a baseline value for their headache and after the tests have been scored. That due to the large number of applicants some patients would have to wait until the first group had completed treatment and that assignment to the first or second group would be done at random. That daily headache diaries had to be kept during the period in which the study was carried out, and that there would be a follow-up session six weeks after completion of the study. (To ensure optimal compliance in coming to the follow-up session, patients were told that those who were interested in their test results could have their test results discussed then.)

Finally, patients were told that each therapy session would consist of two parts: The first half would consist of discussing personal subjects of an emotional nature with

the therapist (e.g. problems, sources of tension) and that the second part of each session would consist of doing either the concentration or the relaxation exercises. The rationale we gave was: If you manage to do the exercises in a moderately aroused state, it will be easier to do them at home in a less aroused state, during the acquisition phase; and that as soon as the patient has become proficient in doing the exercises they should also be done at home when feeling arousal, tension, in order to break the chain of events that leads to the headache.

4.1.4.2. Therapist instructions

In essence therapist instructions were a more sophisticated version of the rationale given to the patients (a copy is included in Appendix A). In order to make the study not only a double blind one, but also a study on the effects of therapist bias, the following elements which were not included in the rationale given to the patients were added: Therapists were told that research in the U.S.A. had established that certain personality types benefited more by concentration therapy and others more by relaxation therapy. Their design, however, prevented a definitive conclusion, although results were highly significant: They had not used a factorial design.

In order to prove conclusively that it was indeed the assignment procedure that increased therapy effectiveness, one half of the patients for whom therapy R was indicated should get therapy R, and the other half should be given therapy C; and that the same should be done with patients for whom therapy C is indicated. And that the purpose of our study was to do this. In reality, however, assignment to the conditions was done at random.

4.1.4.3. Treatment

Treatment consisted of 8 weekly sessions lasting one hour each. The first half of each session consisted of verbal therapy, the second part of either relaxation or concentration exercises.

The first session was an exception to this rule: Here, during the first part of the therapy hour, the therapist informed the patient which kind of therapy she would get, what she could expect from the verbal therapy part of each of the sessions to come, and what was expected of the patient (such as daily home practices).

4.1.4.3.1. Verbal therapy

The first part of sessions 2 - 8 consisted of discussing emotionally loaded personal issues in a non-directive way (on the part of the therapist). This was done on the basis of items contained in the NSC (Nawas Sentence Completion, see Appendices B and C), one of the four "tests" each patient had filled out before entering treatment.

The NSC is a sentence completion test consisting of 56 items, covering 7 categories, viz.:

1. Despair, hopelessness, guilt & shame
2. Sex and sex roles
3. Attitude towards parents
4. Aspirations, future orientation, self concept
5. Fears, concerns, personal gaps
6. Positive reactivity, potentials for mastery of problems
7. Interpersonal perceptions & values.

Each category is represented in the text by 8 items. During each session one such category was used as a focal point of the verbal therapy part of the session. From the answers the patient had given to the individual items the therapist chose those for discussion which he expected would produce a moderate level of arousal in the patient. Therapists were

explicitly instructed not to choose items that were likely to cause the patient so much arousal or distress that it could not be handled in one session.

4.1.4.3.2. Relaxation therapy

The relaxation treatment consisted of progressive relaxation exercises. A treatment manual was given to each patient after session one, as a guide to the daily home practice (see Appendix H).

Patients were instructed to tense each muscle group about three seconds and then relax, concentrating on the feelings in the muscles for about ten seconds. They were asked to repeat the exercise after a relaxation period of about 30 seconds. The therapist gave verbal instructions throughout the session. After completion of the exercises patients were instructed to remain in the relaxation chair focusing on the feeling of relaxation for about five minutes.

During the first two sessions all muscle groups in the manual were used, and most of the exercises were done twice. In session three and four all exercises were done only once. The time that was "saved" by this, was added to the time spent in relaxation after completion of the exercises. During session five and six the exercises for the hands and arms were left out, and during the last two sessions the exercises for the leg muscles were deleted. Each time that exercises were deleted the extra time was added to the time spent in relaxation after completion of the exercises. This way the patient learned to relax in a progressively shorter period of time. Patients were instructed to practice these exercises at least once a day, and at least until the follow-up session.

4.1.4.3.3. Concentration therapy

The concentration exercises were devised by the author as a control treatment, and contained all the noncritical ele-

ments of the relaxation training, except the muscle relaxation: e.g. practice during the second half of each therapy hour and daily at home for an equal amount of time as in the relaxation treatment, verbal instructions by the therapist, graded structure of the exercises, etc.

The individual exercises were adapted from a book containing concentration exercises for pupils/students with concentration problems at school (Langedijk, 1978), and from concentration exercises used in Yoga (no meditation exercises were included, because these may result in a state of relaxation akin to that obtained through progressive relaxation). The concentration exercises used in this study are quite similar to the "placebo" control pseudo-meditation therapy Holroyd, Andrasik & Noble (1980) used in their (single blind) headache study.

In contrast to the relaxation therapy, with patients lying in the relaxation chair during the exercises, patients remained seated (in the same relaxation chair) during the concentration exercises. During sessions 1 and 2 the "Phase I" manual (see Appendix I) was used containing the easiest exercises, during sessions 3 and 4 the "Phase II" manual (see Appendix J), and during sessions 5 and 6 the "Phase III" manual (see Appendix K). In sessions 7 and 8 a modified form of the "Phase III" manual was used (the first exercise was deleted). Patients received copies of the treatment manuals as a guide to home practice (The "Phase I" manual was given after session 1, the "Phase II" manual after session 3, and the "Phase III" manual after session 5).

The concentration exercises were given this way (several phases, progressing from relatively easy to more difficult exercises) to give patients the same feeling of accomplishment and progress as in the relaxation therapy.

4.1.4.4. Assignment to conditions

The 68 patients included in the study were assigned to one of five conditions (four treatment conditions and one wait-

ing list group) at random. Demographic characteristics, personality characteristics, measurements related to the headache and pre-therapy expectation of improvement are given in table 4.2.

A schematic diagram of the tension headache project, also giving information about the number of patients involved in each stage, and the number of dropouts, is contained in figure 4.1.

4.1.5. SOME METHODOLOGICAL CONSIDERATIONS

- Patients were assigned to therapists at random, with the result that each therapist saw patients from all four treatment conditions. This was done to prevent the confounding of therapist and treatment effects.
- The daily headache diaries had to be sent to the research team weekly, and reminders were sent to patients when they were three days late. This was done to prevent data loss from occurring for prolonged periods of time, due to patients forgetting to fill out the headache diary cards.
- During the first hour of therapy patients were told by their therapist, that the data collection and analysis was done by a seperate research team, and that they should keep sending the headache diaries weekly by mail, instead of bringing them to the therapist at the weekly sessions. This was done to ensure optimal accuracy in symptom monitoring: Patients should not feel constrained in reporting headache activity by the thought that the therapist had access to these data during therapy.
- The evaluation form filled out by each patient at follow-up was completed right before the follow-up session in the presence of a research assistant, who was not informed about the treatment condition the patient had been in. This was done to prevent experimenter bias from occurring.
- Therapists received information about the condition each patient was in together with other information about the pa-

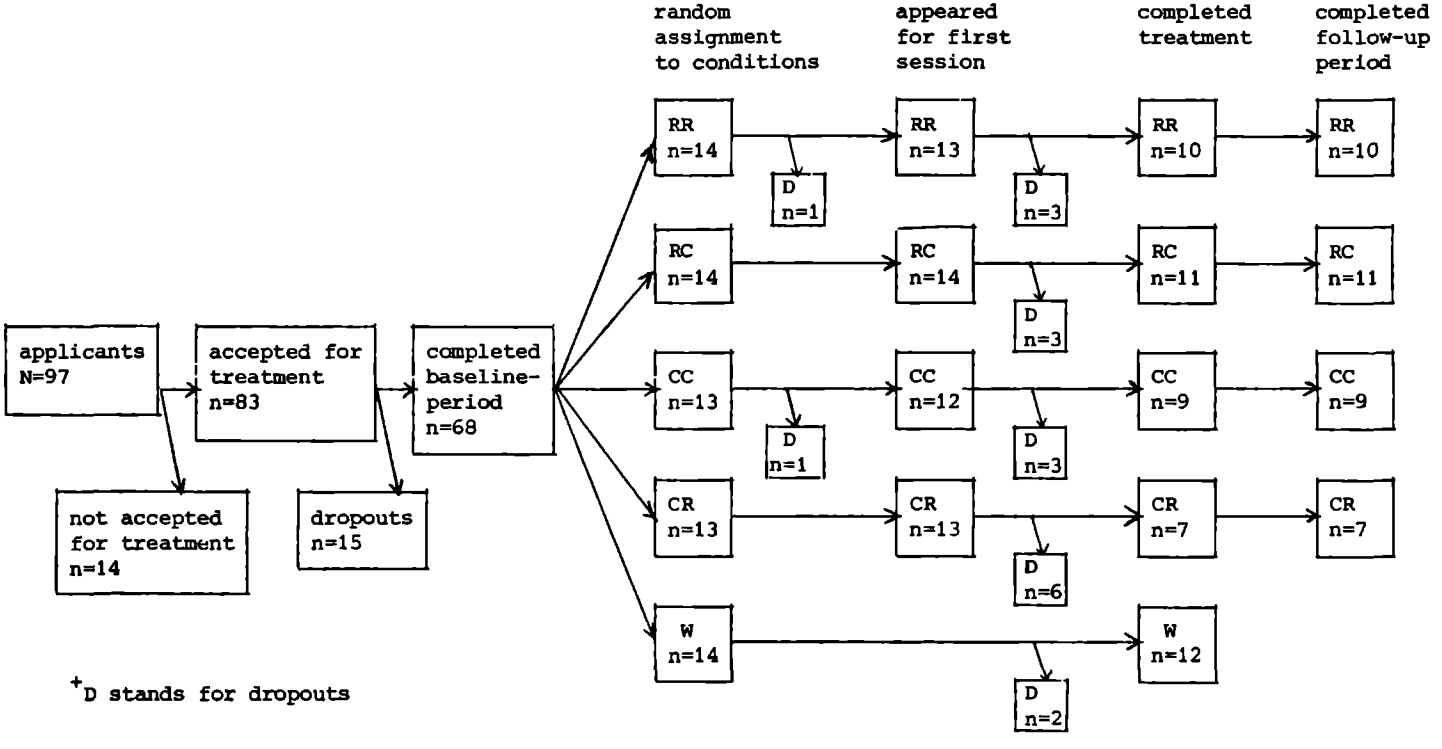
Table 4.2.: Pre-treatment scores of the patients in the five conditions on a number of demographic, personality, and headache variables (experiment I)

	Total	RR	RC	CC	CR	W	F	p
Patients	n=68	14	14	13	13	14	-	-
Age	$\bar{x}$=32.59	32.33	32.64	32.15	33.23	31.64	0.096	.983
	sd= 8.18	8.06	10.13	7.41	8.28	7.82		
Education[1]	$\bar{x}$= 4.91	4.86	4.86	5.23	4.54	5.07	0.141	.966
	sd= 2.45	2.80	2.48	2.49	2.50	2.30		
N.P.V. raw scores								
IN	$\bar{x}$=20.03	17.67	20.93	17.92	20.84	22.36	0.665	.619
	sd= 9.01	10.33	6.18	9.72	9.74	9.25		
SI	$\bar{x}$=14.44	16.33	15.86	14.08	12.54	13.50	0.601	.664
	sd= 7.33	7.49	7.44	8.47	6.41	6.82		
RG	$\bar{x}$=24.38	24.42	24.36	23.08	25.23	24.79	0.101	.982
	sd= 8.88	8.14	7.65	11.72	8.06	9.49		
VE	$\bar{x}$=19.44	20.42	18.64	17.38	21.46	19.43	0.459	.766
	sd= 8.23	8.04	7.20	8.52	9.06	8.90		
ZE	$\bar{x}$= 9.70	12.92	7.21	9.62	10.54	8.71	2.364	.063
	sd= 5.17	6.82	3.66	4.25	6.10	3.47		
DO	$\bar{x}$=11.95	9.92	10.79	13.23	13.85	11.93	1.125	.353
	sd= 5.53	5.47	6.83	5.46	5.38	4.03		
ZW	$\bar{x}$=22.55	20.25	21.43	24.46	23.38	21.21	1.277	.289
	sd= 7.17	8.10	6.15	7.07	6.96	7.29		
MMPI raw scores								
L	$\bar{x}$= 5.03	4.71	5.43	5.38	5.00	4.64	0.467	.760
	sd= 1.95	2.13	2.06	1.98	1.87	1.86		
F	$\bar{x}$= 7.75	7.71	7.43	5.77	7.92	9.79	1.356	.259
	sd= 4.57	4.20	3.27	2.92	5.30	6.08		
K	$\bar{x}$=12.68	12.29	12.86	13.69	12.15	12.43	0.325	.860
	sd= 3.88	3.75	4.50	3.04	5.15	2.98		
Hs	$\bar{x}$=15.07	13.43	15.07	14.23	16.92	15.79	0.954	.439
	sd= 5.08	4.88	4.36	6.55	4.48	4.95		
D	$\bar{x}$=26.85	27.14	27.21	26.92	25.31	27.57	0.280	.890
	sd= 5.94	4.93	5.42	7.75	5.71	6.28		
Hy	$\bar{x}$=32.24	30.36	32.93	32.00	32.92	33.00	0.625	.646
	sd= 5.27	4.40	5.43	6.15	4.52	5.95		
Pd	$\bar{x}$=20.90	19.50	22.50	17.69	21.46	23.14	1.622	.180
	sd= 6.59	4.97	7.20	5.53	7.08	7.20		
Mf	$\bar{x}$=38.00	38.21	40.14	37.62	36.38	37.50	1.946	.114
	sd= 3.76	4.04	3.03	4.31	3.43	3.37		

	Total	RR	RC	CC	CR	W	F	p
Pa	$\bar{x}$=12.62	12.00	12.93	11.38	12.69	14.00	1.038	.395
	sd= 3.59	4.61	3.38	2.43	3.75	3.33		
Pt	$\bar{x}$=20.97	20.29	22.21	19.08	19.62	23.43	0.717	.584
	sd= 7.93	8.63	7.66	8.62	7.12	7.83		
Sc	$\bar{x}$=21.07	19.93	19.71	18.62	20.46	26.43	1.635	.177
	sd= 9.16	8.06	8.68	8.68	8.57	10.70		
Ma	$\bar{x}$=16.51	14.93	15.64	15.92	16.54	19.50	2.532	.049
	sd= 4.35	3.47	3.18	4.87	4.99	4.09		
Si	$\bar{x}$=34.99	38.21	35.36	33.77	33.08	34.29	0.676	.611
	sd= 8.96	9.19	10.35	8.80	7.60	8.95		
H.duration[2] (in years)	$\bar{x}$=10.29	9.29	9.07	11.31	11.23	10.71	0.375	.826
	sd= 6.33	6.08	5.38	7.13	6.80	6.80		
H.severity	$\bar{x}$= 8.00	8.29	7.79	8.08	7.85	8.00	0.091	.985
	sd= 2.38	1.86	2.39	2.84	2.44	2.60		
Baseline measures:								
H.index	$\bar{x}$=230.64	145.83	206.85	341.96	237.70	223.28	1.991	.107
	sd=187.43	105.74	137.52	263.96	180.82	185.24		
medication	$\bar{x}$= 2.79	1.35	3.51	3.54	3.49	1.96	0.855	.496
	sd= 4.00	1.30	3.69	4.57	5.23	4.04		
H.days[3] p.w.	$\bar{x}$= 3.79	2.98	3.95	4.33	3.74	3.86	0.876	.484
	sd= 1.85	1.52	1.42	2.21	1.66	2.26		
H.duration p.w. (hours)	$\bar{x}$=39.58	25.65	34.54	53.99	40.99	41.88	1.774	.146
	sd=28.45	17.09	19.24	39.22	26.20	30.91		
H.intensity	$\bar{x}$= 5.06	4.86	5.21	5.23	5.29	4.72	0.222	.925
	sd= 1.92	1.38	1.66	2.61	1.83	2.13		
Expected[4] results	$\bar{x}$= 3.72	3.29	3.83	3.58	3.69	4.21	0.981	.425
	sd= 1.28	1.32	1.64	1.08	1.32	0.98		

1) To determine the educational level of the patient, the van Westerlaak, Kropman, & Collaris (1975) scoring system was employed. It contains nine categories (1= lowest educational level, elementary school or equivalent; 9= highest educational level, graduate university degree).
2) H.= Headache
3) p.w.= per week
4) The BV (see 4.1.3.1.) contained a question about the amount of improvement patients expected to gain through participation in the study. They answered this question on a seven-point scale (1= totally gone; 2= almost totally gone; 3= much less; 4= about half less; 5= a little less; 6= the same; 7= worse).

Fig. 4.1.: Schematic diagram of the various phases of the headache treatment project (Experiment I), including information about patient flow during the study



tients assigned to them. This was contained in a "patient record" sheet (see Appendix L) giving the therapist information about the age, address, name, etc. of the patient, plus the condition she was in (RR, RC, CC, or CR).

4.2. RESULTS

The following questions are to be answered with this experiment:
a. Is double blind research feasible in psychotherapy?
b. Is double blind research desirable cq. necessary in psychotherapy? This second question was formulated more concretely as follows: Does therapist bias affect outcome?

4.2.1. THE SINGLE BLIND ASPECT

Before we can address ourselves to the first question we must first show that the experiment was at least single blind in nature. In psychotherapeutic research (when the procedural similarity criterion has been met; see par. 3.4.) this amounts to checking whether the patients perceived the two therapies used (relaxation and concentration therapy) as having equal standing, i.e. whether the two therapies were equally credible.

To test the credibility of both therapies the following question was included in the Patient Evaluation Form (see Appendix F) at follow-up:

> Would you recommend the treatment that you received to an acquaintance with tension headache who asks you what he could do best to alleviate his complaint?
>
> 1. definitely yes
> 2. probably yes
> 3. possibly yes
> 4. I am not sure yet
> 5. possibly not
> 6. probably not
> 7. definitely not

By one-way analysis of variance (Winer, 1971) the answers of all of the patients who had received relaxation therapy

(conditions RR and RC) were compared to those of all patients who had received concentration therapy (conditions CC and CR). The results in table 4.3. show no significant difference in the credibility ratings of the two treatments.

Table 4.3.: Patient therapy-credibility ratings
(1 = most credible; 7 = least credible)

Therapy	n	$\bar{x}$	sd	df	F	p
Relaxation	21	1.88	0.85	1,34	0.443	.513
Concentration	15+	2.07	1.03			
Total	36+	1.99	0.92			

+One missing value

The Patient Evaluation Form also contained two open-end questions concerning the perceived attractiveness of the therapeutic modality they had received. Patients were asked to state which aspect(s) of the therapy that they had received they liked best and which aspect(s) they liked least. From the 21 patients who had received relaxation therapy 11 specified the relaxation exercises as being the aspect they liked best, and for 2 patients the exercises were the aspect they liked least. For the patients who had received concentration therapy the numbers were 8 and 1 respectively. Spitz's L-Test [1] showed no significant difference between the two therapies in this respect(Alpha .05 [2], L=0.086, df=1).

1) The L-Test is a likelihood ratio test. It is used to test whether the distributions of two or more populations are proportionally equal, just like the chi-square test. However, the chi-square test cannot be used when one or more of the cells contain(s) less than 5 observations. The L-Test does not have this disadvantage. In all other respects the two tests are more or less equivalent (see Mood, 1950: 273-280; Spitz, 1961: 68-88; Spitz, 1965: 381-384; and Woolf, 1957: 397-409).

2) Throughout this study alpha will always be .05, and statistical significance testing will be performed two-tailed.

These results may be interpreted as providing additional evidence that the patients considered the two therapies as being of equal standing. Therefore, it may be concluded that because the two therapies were rated by the patients as being equally credible and attractive, the single blind aspect of the study has been realized.

4.2.2. THE DOUBLE BLIND ASPECT AND THERAPIST BIAS

The question "Is double blind research feasible in psychotherapy" can now be addressed. This question is largely a theoretical, methodological problem, and has, as such, been discussed in chapter 3, par. 3.5. and the methods section of this experiment. We showed that a design can be conceived of, in which therapist blindness is created. However, it cannot be assumed that therapist blindness has indeed been realized when this design has been implemented: It remains to be checked if the manipulations outlined in the methods section had the desired effects. The question is: Did the therapists actually accept the rationale (see Appendix A) given to them? Obtaining an answer to this question is not an easy task, however: Direct questions concerning this issue may breed "suspicion" about the veracity of the rationale in the minds of the therapists, and defeat their own purpose. A second reason is, that a more elaborate replication study was planned, in which the therapists would be recruited from the same population as the therapists used in this experiment (graduate students of psychology): Should a too direct questioning concerning the double blind aspect produce therapist suspicion, a second experiment might become impossible because of this; the suspicion of the therapists of the first experiment might then be communicated to the therapists of the second experiment.

Therefore, it was decided to obtain the necessary information in a more indirect, less obtrusive and less reactive way. The Therapist Evaluation Form (see Appendix G), com-

pleted by each therapist a few days after having seen the last patient for the last time, contained the following questions which were relevant to this purpose:

a. What is your opinion of the design and organization of this research?
b. Do you have any suggestions regarding the design of this research?
c. What is your position concerning the ethical aspects of employing experimental treatments, control treatments and waiting list control groups as used in this study?
d. Do you have any suggestions or comments that have not been covered above?

The questions were posed in this open-end fashion in order to give each therapist the opportunity to answer whatever they felt important enough to report. This means that the answers will probably contain information that is incidental to our purpose. Furthermore, since therapists have the opportunity to answer in idiosyncratic ways, a question-by-question comparison is problematic and statistical significance testing impossible. Therefore, it was decided to evaluate the answers as follows: First the answers were checked on positive evidence for therapist "suspicion" about the rationale of the design. Then the same was done for positive evidence that the therapists had accepted the rationale.

Typically, the dominant themes of the answers were not relevant to our purpose: Therapists commented on the technique-centeredness of the study, expressed a desire for continued supervision during the period they did the therapies, the desirability of mutual contacts between therapists, so that they could exchange experiences, etc. The answers did not contain any statements even remotely suggesting that therapists had reservations about the rationale given to them. The answers did, however, contain positive evidence that they had indeed accepted the rationale. The parts of the statements containing this kind of information are listed in Table 4.4.; therapist by therapist, beginning with the three female therapists (F 1,2,3) and proceeding with the answers of the three male therapists (M 1,2,3).

Table 4.4.: Therapist comments concerning the design of the study

Therapist	Comment
F1	"I found it very disturbing to give somebody the wrong *(CR or RC conditions, F.W.)* therapy. Because they trust you, and in reality this trust is not warranted."
F2	"I find it *(controlled research of this type, F.W.)* very unethical. The next time I would not participate again."
F3	"I keep finding it problematic, especially if somebody in the wrong condition does not improve. To improve psychotherapy, however, it is desirable to do research in this field. .. In my opinion it is better not to know which condition a patient is in." *(sic!, F.W.)*
M1	"In the CR condition patients often got headaches during the exercises in the first weeks of the therapy, because they did the exercises too tensely. Other therapists also noticed this. .. In the control therapy conditions *(CR and RC, F.W.)* I tended to pay more attention to the verbal part of the therapy."
M2	"I do not see how you can do good research in psychotherapy in any other way. Also, many patients already benefit from the attention they receive from the therapist. And even if they do not improve, at least the therapy does no harm."
M3	"The research is well designed. .. After thinking it over, I do not have major problems with it *(the ethical aspects of control treatment groups, F.W.)* anymore."

An informal talk with each therapist individually after completion of the evaluation form, in which the experimenter in a non-directive way elicited comments on the part of the therapists, a.o. concerning the design of the study, confirmed the data reported in the preceding section.

It may therefore be concluded that there is no reason to assume that (some of) the therapists had reservations or suspicions about the rationale given to them, and that the double blind aspect of the study has been realized. Furthermore, because the rationale not only contained information designed to produce therapist blindness but also to produce therapist bias (see par. 4.1.4.2.), the evidence in this

section which showed that the rationale had been accepted by the therapists, also shows that therapist bias has been implanted successfully.

4.2.3. THE INFLUENCE OF THERAPIST BIAS

The second question which is to be answered by this experiment ("Is double blind research desirable cq. necessary in psychotherapy?") concerns therapy outcome and the influence of therapist bias on it.

The decision was made to use the Headache Index (see par. 4.1.3.2.) – the most widely used and most sensitive parameter in measuring change in headache symptomatology – to measure outcome. Baseline headache index levels were computed by averaging the weekly headache index for the last four weeks of the baseline (see footnote on p.36). Post-treatment values were obtained by calculating the weekly average of the last two weeks of treatment, and follow-up values were computed on the basis of the weekly average of the last two weeks of the follow-up period. A survey of the changes in the headache index in the four treatment conditions, compared to change in the waiting list control group is contained in table 4.5.

The one-way analysis of variance of the baseline data in table 4.2. was computed for all patients, including the dropouts. The figures in table 4.5. are based only on patients who remained in therapy. Therefore, a one-way analysis of variance was again performed on the baseline values for the five experimental groups, to check whether the groups were still equivalent, after the dropouts had been deleted. No significant <u>overall</u> differences were found ($df=4,44$, $F=1.405$, $p=.248$). However, the Multiple Range Test, in which a <u>pairwise</u> comparison was done, established that a significant difference between the CC and RR group existed at the .05 level. The substantial differences which occurred between the groups on baseline headache index val-

Table 4.5.: Headache indices for experiment I[+]

Condition	Baseline	Post-treatment	Follow-up
RR	$\bar{x}$= 135.40	$\bar{x}$= 43.29	$\bar{x}$= 61.95
n=10	sd= 106.58	sd= 62.41	sd= 54.16
RC	$\bar{x}$= 224.61	$\bar{x}$= 123.34	$\bar{x}$= 136.86
n=11	sd= 146.91	sd= 88.27	sd= 119.04
CC	$\bar{x}$= 321.80	$\bar{x}$= 264.36	$\bar{x}$= 131.56
n=9	sd= 234.74	sd= 121.77	sd= 92.91
CR	$\bar{x}$= 223.15	$\bar{x}$= 174.87	$\bar{x}$= 156.44
n=7	sd= 191.38	sd= 177.69	sd= 143.62
W	$\bar{x}$= 207.80	$\bar{x}$= 238.06	
n=12	sd= 172.67	sd= 189.98	

[+] Variances of the headache index scores are quite high. This is due to the fact that the headache index is a "composite" score, with a possible range of 0-2016.

Table 4.6.: Differences in headache indices between the five conditions post-treatment

Source of variation	df	Mean square	F	p
Covariate baseline	1	357691.69	26.266	.000
Main effects condition	4	46106.45	3.386	.017
Explained	5	108423.50	7.962	.000
Residual	42	13617.89		
Total	47	23703.59		

N.B.: 49 Cases were processed, 1 case of which was missing (due to failure of one patient in the CC condition, who did complete treatment and did come to the follow-up session, to fill out the headache diary cards).

ues, in spite of the random assignment to conditions, may obscure a clear assessment of treatment effects. Therefore, it was decided to employ analysis of covariance (with baseline as covariate) for subsequent analyses on post-treatment and follow-up headache index data. This method of analysis eliminates the variance which can be attributed to group differences already existing at the baseline from the post-treatment and follow-up variance post hoc.

Then the data analysis focused on establishing whether any differences existed between the five groups (the four treatment groups plus the waiting list control) after the completion of treatment. This was done by analysis of covariance (with baseline values as covariate, and groups as main effect).

The results in table 4.6. clearly show that there is a significant difference between the five conditions at post-treatment for the headache index values (after correction for differences in baseline values).

The next step was to investigate which of the four treatment groups differed significantly from the waiting list control at post-treatment. A pairwise comparison of each of the four treatment conditions with the waiting list control (also by analysis of covariance) showed that only the two groups receiving relaxation therapy (the RR and RC conditions) differed significantly from the waiting list control at post-treatment (see table 4.7.).

Table 4.7.: Headache index at post-treatment: A pairwise comparison of the treatment groups with the waiting list control, corrected for baseline differences

Comparison	df	F	p
RR vs. W	1,19	13.234	.002
RC vs. W	1,20	8.249	.009
CC vs. W	1,17	0.226	.641
CR vs. W	1,16	1.109	.308

These results indicate that, no matter what therapists believe about their relative efficacy, relaxation therapy is consistently superior to the waiting list control, while the concentration therapy both under negative and under positive therapist bias is not significantly better than the waiting list control at post-treatment. Apparently the treatment given (relaxation therapy vs. concentration therapy) is more important in producing the results than therapist bias (positive therapist bias, viz. conditions RR plus CC, versus negative therapist bias, viz. conditions RC and CR).

This was confirmed by a factorial analysis of covariance (2 x 2 design, with covariate Baseline), investigating the effects of Treatment Given (relaxation therapy or concentration therapy) and Therapist Bias (positive, RR + CC and negative, RC + CR). See tables 4.8. and 4.9.

Table 4.8.: Analysis of covariance for the effects of Treatment Given (2) x Therapist Bias (2) on post-treatment headache index (with baseline headache index as covariate)+

Source of variation	df	mean square	F	p
Within + residual	28	11829.29		
Constant	1	730625.89	61.764	.000
Baseline within TG x TB	4	51366.49	4.342	.007
Treatment Given (TG)	1	72415.77	6.122	.020
Therapist Bias (TB)	1	5.26	0.000	.983
TG x TB	1	25699.86	2.173	.152

+ This analysis (as well as the analysis in table 4.9.) was done with the aid of program MANOVA, analysis of covariance with seperate regression estimates, model 3 (see Hull & Nie, 1981: 15-16).

By inspecting table 4.8. it becomes clear that only TG (Treatment Given) had a significant effect on outcome at post-treatment. Neither Therapist Bias (TB) nor the interaction between TB and TG produced significant effects. These results were maintained at follow-up (see table 4.9.).

Table 4.9.: Analysis of covariance for the effects of Treatment Given (2) x Therapist Bias (2) on follow-up headache index (with baseline headache index as covariate)

Source of variation	df	mean square	F	p
Within + residual	28	9037.69		
Constant	1	507074.48	56.107	.000
Baseline within TG x TB	4	16000.27	1.770	.163
Treatment Given (TG)	1	49385.91	5.464	.027
Therapist Bias (TB)	1	20321.27	2.249	.145
TG x TB	1	12644.10	1.399	.247

Another way of approaching the same issue is by using change scores instead of analysis of covariance. This procedure is commonly used in the reports on headache treatment published in recent years (see e.g. Blanchard, Andrasik, Neff, Arena, et al., 1982). The change scores are divided in three categories: much improved (50% or more improvement), slightly improved (20-49% improvement), and unimproved or worse (less than 20% improvement). From a purely statistical point of view this is clearly a less elegant procedure than analysis of covariance (Cronbach & Furby, 1970). However, its results are more readily interpretable and change scores provide clinically significant information not contained in the data produced by analysis of covariance: It provides the clinician with information about the frequency with which patients experience clinically meaningful reductions in headache activity (Hugdahl & Ost, 1981). Furthermore, this type of analysis allows for the incorporation of dropouts in the analysis, which cannot be done with analysis of covariance.

Tables 4.10. and 4.11. contain the change scores (computed on basis of the headache indices) for the various conditions at post-treatment (table 4.10.) and at follow-up (table 4.11.). In order to investigate the effects of Treatment Given (relaxation and concentration therapy), the data for the RR and RC conditions have to be collapsed into R (relax-

ation, and the data for the CC and CR conditions into C (concentration).

Table 4.10.: Post-treatment change scores

Condition	n	much improved (≥50%)	slightly improved (20-49%)	unimproved or worse (<20%)
RR	10	8	2	0
RC	11	5	4	2
CC	9+	2	2	4
CR	7	2	0	5
W	12	0	3	9
Relaxation (RR+RC)	21+	13	6	2
Concentration (CC+CR)	16	4	2	9
Positive TB++ (RR+CC)	19+	10	4	4
Negative TB (RC+CR)	18	7	4	7

+One value missing

++TB stands for Therapist Bias

Table 4.11.: Follow-up change scores

Condition	n	much improved (≥50%)	slightly improved (20-49%)	unimproved or worse (<20%)
RR	10	5	2	3
RC	11	5	4	2
CC	9+	5	1	2
CR	7	3	1	3
Relaxation (RR+RC)	21+	10	6	5
Concentration (CC+CR)	16	8	2	5
Positive TB++ (RR+CC)	19+	10	3	5
Negative TB (RC+CR)	18	8	5	5

+One missing value

++TB stands for Therapist Bias

At post-treatment the three groups (relaxation, concentration, and the waiting list control) were significantly different (L-test: L=24.488, df=4, $p<.001$). A pairwise comparison gave the following results: Relaxation therapy was significantly superior to concentration therapy (L=10.92, df=2, $p<.005$), and the waiting list control (L=21.374, df=2, $p<.001$). Concentration therapy did not differ significantly from the waiting list control (L=5.412, df=2, $p>.05<.10$).

At follow-up the difference between concentration therapy and relaxation therapy was not significant anymore (L=1.31, df=2, n.s.). However, concentration therapy produced more dropouts than relaxation therapy (9 vs. 6) and none of the dropouts at a phone contact after they dropped out of treatment, indicated that the reason for dropping out had been a significant decrease in headache symptomatology. Therefore, it may be argued that they can be classified as having improved less than 20%. But even then no significant difference was found (L=2.50, df=2, n.s.) between relaxation and concentration therapy at follow-up.

To investigate the effects of therapist bias (positive vs. negative), the RR and CC conditions have to be collapsed into one category (positive therapist bias) and the RC and CR conditions into another one (negative therapist bias): RR and CC being the two conditions in which the therapists believe that the patient gets the therapy she needs, and RC and CR the conditions in which the therapists believe that the patient does not receive the therapy she needs. No significant differences were found between the Positive Therapist Bias (RR + CC) and the Negative Therapist Bias (RC + CR) groups at post-treatment (L=1.356, df=2, n.s.) or at follow-up (L=0.726, df=2, n.s.). Also, when dropouts were considered as having improved less than 20%, these results were maintained at post-treatment (L=1.75, df=2, n.s.) as well as at follow-up (L=0.912, df=2, n.s.). Therefore, it may be concluded that, at least on the basis of the headache index scores, relaxation therapy consistent-

ly emerges as the superior treatment at post-treatment, no matter what the therapists believe about its appropriateness for a particular patient. At follow-up the analysis of covariance still showed relaxation therapy to be superior, but the analysis on the basis of change scores did not: Although the proportion of patients who benefited substantially from relaxation treatment was still greater than the proportion of patients who benefited substantially from concentration therapy, this difference failed to reach statistical significance.

Another important result obtained is that apparently therapist bias does not significantly influence outcome. This suggests the conclusion that double blind research is perhaps not necessary in psychotherapy. However, this conclusion is based upon the results obtained with the headache index data as dependent variable. The headache index is quite a "hard" outcome measure, that does not require post hoc subjective judgements from either patient or therapist. In psychotherapeutic research such nice, clearcut and objective outcome data are usually not available (at least not used). This is mainly so because the subjective symptoms with which psychotherapy is primarily concerned (depression, anxiety, interpersonal problems, etc.) are not easily translated into a measure as objective as the headache index. Research in psychotherapy commonly utilizes global judgements of effect, given at post-treatment by either patient or therapist or both as measures of outcome; subjective post hoc therapist judgement being by far the most commonly used measure of therapy outcome (Garfield, 1980; Mintz, 1977; Rachman & Wilson, 1980).

If we had used these commonly used measures of outcome (subjective patient and therapist judgements) would the conclusion reached above still hold? Let us first take a look at the outcome results obtained through global therapist judgement of outcome. At the post-treatment evaluation session with each individual therapist, each of them filled

out a form that among others contained the following two questions:

a. Which of the patients who you have treated (dropouts excluded) improved most?
   Names of patients: .....
b. Which of the patients who you have treated (dropouts excluded) improved least?
   Names of patients: .....

From these data we can compute the number of patients who were judged as having improved most and least in the four treatment conditions. These results are presented in table 4.12.

Table 4.12.: Global therapist judgement of therapy outcome

Condition	most improved	least improved
RR	6	0
RC	3	8
CC	7	2
CR	1	4
Relaxation (RR+RC)	9	8
Concentration (CC+CR)	8	6
Positive TB+ (RR+CC)	13	2
Negative TB (RC+CR)	4	12

+ TB stands for Therapist Bias

For investigating the effects of treatment given (relaxation or concentration) the RR and RC conditions must be collapsed into one category and the CC and CR conditions too. In order to investigate the effects of therapist bias the same was done with CC and RR (positive therapist bias) and RC and CR (negative therapist bias). Comparing relaxation (RR +RC) with concentration (CC+CR) no significant difference between them is found (L=0.056, df=2, n.s.). However, when the positive therapist bias group (RR+CC) is compared with the negative therapist bias group (RC+CR), a significant

difference is seen (L=12.908, df=2, p<.001).

The above results strongly suggest that apparently to a large extent the therapist sees the results he expects to see: Significantly more improvement is reported for the conditions in which he believes that the patients receive the appropriate therapy compared to the conditions in which he believes that the patients get the wrong therapy! Moreover, the therapists see no significant difference in the effectiveness of relaxation and concentration per se, totally in line with their belief that both are equivalent, bonafide therapies. All this is a complete reversal of the results obtained with the headache index data as outcome criterion.

It is now time to look at the global patient ratings of improvement. This information was collected at the follow-up evaluation, when patients filled out a form that included the following question among others:

How is your headache lately?
1. totally gone
2. almost totally gone
3. much less than before treatment
4. about half less
5. a little less
6. the same
7. worse

The results of each of the four treatment groups are presented in table 4.13. A one-way analysis of variance showed no differences between the four groups (df=3,32, F=0.479, n.s.).

Table 4.13.: Global patient ratings of outcome at follow-up

Condition	n	$\bar{x}$	sd
RR	10	3.60	1.17
RC	11	4.18	1.66
CC	8+	3.50	0.93
CR	7	4.00	1.83
Total	36+	3.83	1.40

+One missing value

Table 4.14.: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on global patient ratings of outcome at follow-up

Source of variation	df	mean square	F	p
Main effects	2	1.48	0.715	.497
Treatment Given (TG)	1	0.17	0.083	.775
Therapist Bias (TB)	1	2.69	1.304	.262
TG x TB interaction	1	0.02	0.007	.934
Explained	3	0.99	0.479	.699
Residual	32	2.06		
Total	35	1.97		

A factorial analysis of variance (classic experimental approach; see Nie, Hull, Jenkins, Steinbrenner & Bent, 1975) was performed to investigate the effects of Treatment Given and Therapist Bias. This analysis can be found in table 4.14. The results show that neither Treatment Given (p=.775) nor Therapist Bias (p=.262) had a significant effect on global patient outcome ratings. Also, the interaction of the two did not produce significant results (p= .934). These findings indicate that on the average patients reported an equivalent amount of improvement for the two therapies (relaxation and concentration therapy), and that therapist bias did not have a significant impact on these judgements.

4.3. DISCUSSION AND CONCLUSIONS

The three parameters used in this experiment have produced conflicting results: Had we used global therapist judgement as outcome criterion, we would have concluded that on the average both therapies are equally effective (a finding so commonly encountered in psychotherapeutic research), but significantly less effective when applied to the "wrong" type of patient (both findings totally in line with the bias implanted in the therapists).

Had we relied only on global patient judgement of outcome,

we would have found that both therapies are equally effective, and that therapist bias does not significantly affect outcome. Taking a relatively hard parameter, independent of post hoc judgements by interested parties (patients or therapists), relaxation therapy emerges as the superior therapy (more clearly at post-treatment than at follow-up), and we find no significant effects of therapist bias on outcome.

These findings seriously question the adequacy of global judgements of outcome by either patient or therapist, and plead for devising more objective outcome parameters that do not require global post hoc judgements by participants.

The results of this experiment clearly support the hypotheses that (a) double blind research is feasible in psychotherapy (see 4.2.1. and 4.2.2.), and that (b) at least when the therapist is the one who judges outcome (which is the most common situation in psychotherapeutic research), using the double blind design is necessary (see 4.2.3.).

The objectives of this experiment are the same as those of experiment I. The experimental design, target symptom, and therapies used are identical too. So this experiment is in essence a replication study. The decision to do a replication study was primarily inspired by the consideration that replication studies are important, but very rarely carried out.

To arrive at more clear-cut results, the following additional features were included in this experiment: First of all, the sample of patients now consisted of female tension (or mixed migraine-tension) headache sufferers of about 18-45 years of age, with the additional qualification that tension headache should be the dominant component in the mixed headache cases. This was done to obtain an even more homogeneous sample.

Furthermore, after each session each therapist filled out a Self-Monitoring Form (see Appendix M). This form consisted of a few questions concerning the amount of time spent on each of the two "ingredients" of each session (the verbal part and the exercises). Also, the therapist had to state how well he/she had adhered to the therapy-script during the session. This was done to check out if the therapists had administered the therapies as they had been instructed to, so that afterwards it would be possible to exclude the potential conclusion that differences in outcome might be

attributable to selective differences in therapist activities between conditions. Furthermore, information about the credibility of the two therapies was obtained from the patients at several points during the study, in line with the recommendations of Andrasik & Holroyd (1980), that the assessment of the credibility of therapies should be done after session 1, after the last session, and at follow-up.

In experiment I information about whether the therapists had accepted the rationale (whether they indeed believed that the two therapies were equivalent, and whether therapist bias had been implanted successfully) was collected via indirect questions. In experiment II the experimenter felt secure enough to do some more direct measurements, because there was no indication from the first experiment that therapist suspicion had been raised by questions concerning the acceptance of the rationale.

Finally, the global therapist and patient judgements of outcome were now obtained at the same point in time (at post-treatment), while in experiment I, the patient gave his estimation at follow-up and the therapist at post-treatment. Furthermore, this time global therapist judgement consisted of an evaluation on the same seven-point scale as the one the patient had to answer. In experiment I, the therapist only stated which patients had improved most and which had improved least.

5.1. METHOD

5.1.1. EXPERIMENTAL DESIGN

This was identical to the one used in experiment I (see 4.1.1.).

5.1.2. SUBJECTS

5.1.2.1. Patients

Patients were 75 adult female tension headache (or mixed migraine-tension headache) sufferers, solicited through advertisements in local newspapers. Their mean age was 32.9 years (sd=8.3; range=18-48). The mean duration of headache symptoms was 9.0 years (sd=6.4; range=1-20)[1].

5.1.2.2. Selection of patients

Advertisements were placed in local newspapers, soliciting female tension headache sufferers between the ages of 18-45 to participate in a tension headache treatment project. Respondents (n=134) were invited to a diagnostic interview with the experimenter. Patients were also required to obtain a written statement from their family doctor, confirming that there was no evidence of organic pathology related to the headache and giving his consent for the patient to participate in the treatment.

Because of the larger initial sample as compared to experiment I, it was possible to be even more strict about the admittance criteria: Only patients diagnosed as suffering from tension headache, or mixed migraine-tension headache (with tension headache being the dominant component) were accepted for treatment. This was done to obtain an even more homogeneous sample than in experiment I, in which patients were accepted for treatment with no requirements as to the importance of the tension headache component. Also, as in the first experiment, patients with other headache types and patients who were either overtly psychotic or on antipsychotic medication, were excluded from the study. This limited the number of participants to 96. Eighteen of these 96 patients dropped out during the four week baseline

1) See footnote 2 on p. 35.

period, before assignment to one of the five experimental conditions had taken place, leaving 78 patients. In order to be able to start with groups of equal size, the decision was made to remove 3 patients from the sample at random so that each of the five conditions would contain n=15.

5.1.2.3. Therapists

Six graduate students of psychology were recruited and trained as therapists in the same way as in experiment I. From the 19 applicants, 6 were selected (3 males, 3 females; mean age 25, range 24-26).

5.1.3. INSTRUMENTS

5.1.3.1. Pre-treatment assessment

This was identical to that in experiment I (see 4.1.3.1.).

5.1.3.2. Headache diary

This too was identical to the one used in experiment I (see 4.1.3.2.).

5.1.3.3. Post session 1 assessment

After the completion of session 1 with each individual patient, the therapist filled out a Therapist Evaluation Form (see Appendix N). This form contained 4 questions, the question concerning the therapist's prediction of therapy results being the critical one.

After session 1 the patient filled out an evaluation form (see Appendix O) also containing 4 questions. The critical ones are the questions about which results the patient expects to obtain from the therapy and about her willingness to recommend this treatment to others. Furthermore, both therapist and patient filled out the

Barrett-Lennard Relatievragenlijst (a Dutch revision of the Barrett-Lennard Relationship Inventory, see Litaer, 1976). Both therapists and patients were instructed to convey how they anticipated the therapeutic relationship would develop during therapy (this inventory was included for exploratory purposes, unrelated to the objectives of this experiment; results will be reported seperately).

5.1.3.4. Post-treatment assessment

At post session 8 the same forms as the ones employed at post session 1 were used (see Appendices P and Q), the questions being rephrased in such a way as to elicit information about the current situation.

5.1.3.5. Therapist Self-Monitoring Form

After each session the therapist filled out a short 4-item form (see Appendix M) monitoring the total length of the session, the time spent on the verbal therapy part and the exercises part of the session, and containing the therapist's estimation of how much this session was in accordance with the pre-therapy instructions.

5.1.3.6. Patient Evaluation Form

At the follow-up session, six weeks after completion of therapy, each patient filled out a form (see Appendix R) similar in content to the form filled out at follow-up in experiment I (see Appendix F).

5.1.3.7. Therapist Evaluation Form

Within a week after completion of the treatment of his/her last patient each therapist filled out a Therapist Evaluation Form (see Appendix S), similar in content to the one filled out by the therapists in Experiment I (see Appendix G).

5.1.4. PROCEDURES

5.1.4.1. Patient instructions

The patient instructions were identical to the instructions the patients had received in experiment I (see 4.1.4.1.), with the additional information that this study was a replication study and that not only at follow-up, but also after session 1 and session 8 each patient would be requested to fill out a few, relatively short questionnaires.

5.1.4.2. Therapist instructions

The therapist instructions were identical to the instructions given to the therapists in the first experiment (see 4.1.4.2.) with the following additions: The written rationale contained the additional information that the present experiment was a replication study, but under stricter experimental conditions and that the therapists as well as the patients had to fill out more evaluation forms than in the first experiment (see Appendix A, last page). The following instructions were given to each therapist verbally before the treatment period started, to explain why a number of additional questionnaires had to be filled out in this experiment:

"Because I observed that therapists sometimes had the tendency to pay more attention to the verbal therapy part with some patients, notably patients in the control conditions, each therapist has to fill out a short questionnaire after each session indicating how long was spent on each part of the therapy session (verbal therapy versus exercises) as well as how long the session lasted. Furthermore, after each session you are to indicate to what degree the session was in accordance with the "script" on a seven-point scale. This is done, because some of the therapists in the first experiment took more liberties than others during the ver-

bal therapy part, e.g. by discussing other topics than those contained in the Nawas Sentence Completion, or by spending more time on the verbal therapy part than had been allotted for in the "script". All this is not meant to curtail your verbal therapeutic endeavors excessively, but as a check for us, as to whether the verbal therapy part was adhered to equally strict in all conditions. Of course, you may refrain from discussing the NSC topics indicated by the "script" for a given session if an emergency occurs or if other circumstances warrant a departure from the rules. The point is that we need to know whether this happened or not and to which extent. Also, after session 1 and session 8 both you and the patient will fill out two forms: The first is the Barrett-Lennard, an inventory containing questions about the patient-therapist relationship. This is usually administered only after session 4 because it is very difficult to say anything with any certainty about the relationship before that time. In this study we have decided to administer the Barrett-Lennard after session 1, in order to see if at that time it is already possible to predict how the relationship will develop on basis of the impressions gathered in session 1. After session 8, both you and the patient will fill out the Barrett-Lennard again. This time you will answer it to convey how the relationship has actually developed. The second form is much shorter than the Barrett-Lennard. Here you have to answer four precoded questions, in which you indicate how well you perceived the verbal part went as going. The same you will do with regard to the exercises. Furthermore, on a seven-point scale you will indicate how willing you think the patient is to recommend this treatment to others, and you will give a prediction of how much you expect the patient to improve due to this treatment. The patient will do the same. We want to find out which party is more accurate in predicting therapeutic results. At post session 8 you will answer these questions again, but then you will give your estimation of the results obtained at that point by the patient."

5.1.4.3. Treatment

The treatments used in this experiment were identical to the ones used in experiment I (see 4.1.4.3.).

5.1.4.4. Assignment to conditions

The 75 patients in the study were assigned to one of the five experimental conditions (four treatment conditions and one waiting list control group) at random. Demographic characteristics, personality characteristics, measurements relating to the headache, and pre-therapy expectation of improvement, are given in table 5.1.

A schematic diagram of the tension headache project (experiment II), also giving information about the number of patients involved in each phase, and the number of dropouts, is contained in figure 5.1.

5.1.5. SOME METHODOLOGICAL CONSIDERATIONS

The methodological considerations discussed in 4.1.5. also apply to this experiment.

5.2. RESULTS

As stated before,the objectives of this experiment are basically the same as those of experiment I. The two central questions still are:

1. Is double blind research feasible in psychotherapy?
2. Is double blind research desirable cq. necessary in psychotherapy?

In order to arrive at more clear-cut answers to these questions a number of additional data have been gathered in this experiment.

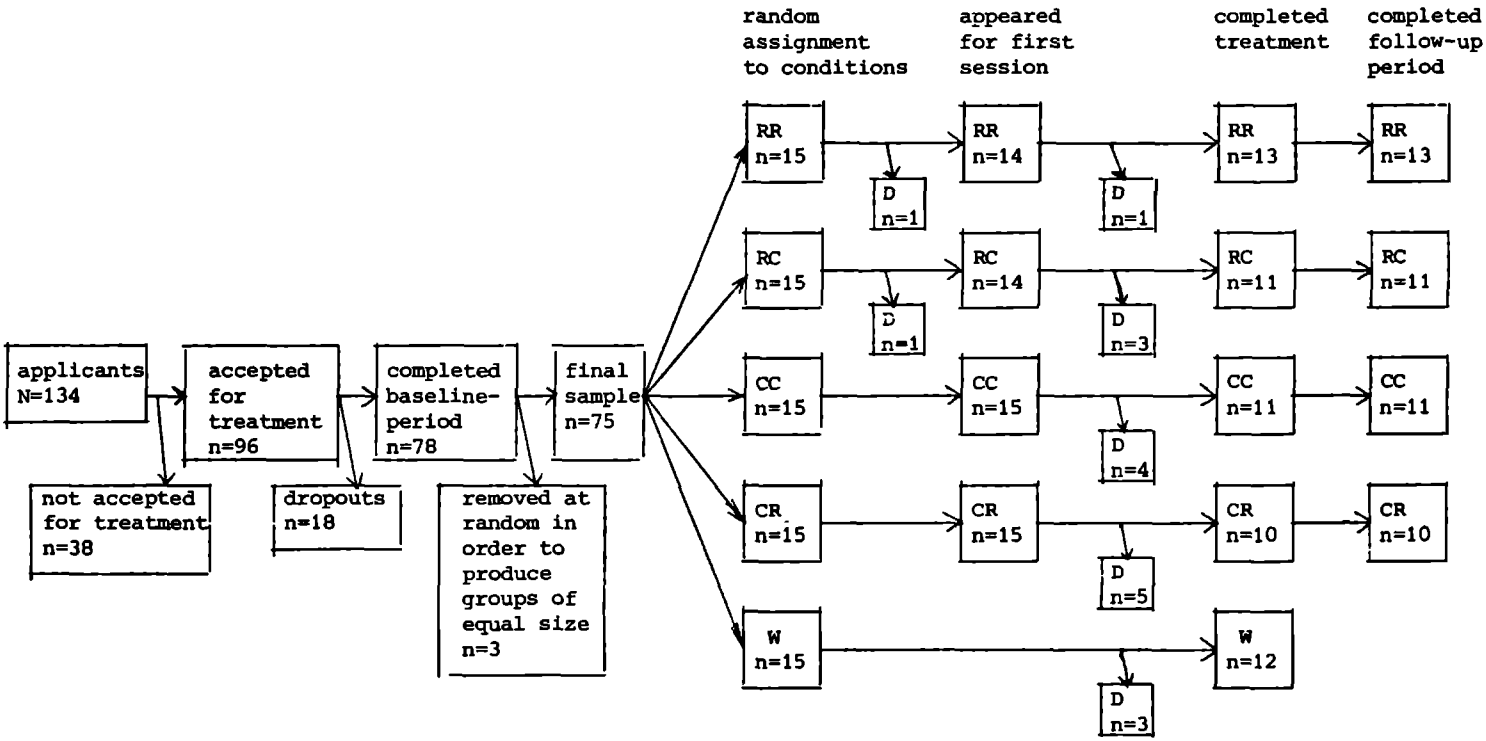
Table 5.1.: Pre-treatment scores of the patients in the five conditions on a number of demographic, personality, and headache variables (experiment II)

	Total	RR	RC	CC	CR	W	F	p
Patients	n=75	15	15	15	15	15	-	-
Age	$\bar{x}$=32.85	32.13	34.07	33.53	34.27	30.27	0.594	.668
	sd= 8.29	8.10	8.45	9.86	7.23	8.08		
Education[1]	$\bar{x}$= 4.64	4.20	4.33	4.13	4.13	3.36	0.403	.806
	sd= 2.25	2.18	2.09	2.00	2.62	2.53		
N.P.V. raw scores								
IN	$\bar{x}$=19.89	18.80	17.73	23.47	16.87	22.60	1.562	.194
	sd= 9.32	8.87	8.07	10.57	10.45	7.54		
SI	$\bar{x}$=14.19	13.87	12.27	17.20	12.40	15.20	1.145	.343
	sd= 7.51	6.60	5.64	8.73	8.27	7.75		
RG	$\bar{x}$=26.37	25.13	26.73	28.47	25.13	26.40	0.476	.753
	sd= 7.62	8.31	6.18	7.59	9.01	7.26		
VE	$\bar{x}$=21.28	17.27	20.47	23.33	20.67	24.67	2.235	.074
	sd= 7.67	10.05	7.20	6.53	7.43	4.98		
ZE	$\bar{x}$=10.37	8.73	11.00	12.67	9.40	10.07	1.249	.299
	sd= 5.34	4.99	6.56	6.23	5.08	2.89		
DO	$\bar{x}$=12.12	12.20	10.93	13.00	13.27	11.20	0.503	.734
	sd= 5.62	4.80	5.75	6.78	6.12	4.75		
ZW	$\bar{x}$=23.76	24.47	23.00	24.53	25.07	21.73	0.709	.588
	sd= 6.24	5.53	5.61	6.98	6.32	6.88		
MMPI raw scores								
L	$\bar{x}$= 6.43	6.13	5.53	5.67	6.40	8.57	0.562	.691
	sd= 6.12	2.36	2.07	3.18	1.76	13.39		
F	$\bar{x}$= 8.03	6.33	6.13	8.93	6.13	12.93	2.683	.039
	sd= 7.13	3.38	3.46	5.36	3.40	13.36		
K	$\bar{x}$=13.03	13.13	13.40	11.67	12.73	14.29	0.322	.863
	sd= 6.33	3.80	2.95	4.53	3.94	12.53		
Hs	$\bar{x}$=16.45	15.80	14.73	18.47	16.80	16.43	1.048	.389
	sd= 5.21	4.86	3.69	5.85	6.65	4.36		
D	$\bar{x}$=27.01	26.73	27.07	27.87	25.53	27.93	0.386	.818
	sd= 5.96	5.15	4.15	7.01	8.06	5.00		
Hy	$\bar{x}$=32.96	31.60	32.53	33.00	34.87	32.79	0.615	.653
	sd= 5.83	5.50	5.29	5.10	7.80	5.31		
Pd	$\bar{x}$=19.74	18.40	17.93	21.73	19.53	21.21	1.370	.254
	sd= 5.57	5.42	4.82	4.88	5.34	6.95		
Mf	$\bar{x}$=37.62	38.07	37.53	38.07	38.60	35.71	1.505	.210
	sd= 3.49	3.35	3.48	3.39	3.14	3.85		

	Total	RR	RC	CC	CR	W	F	p
Pa	$\bar{x}$=11.26	10.80	8.80	13.40	10.47	12.93	4.445	.003
	sd= 3.76	3.21	3.26	3.36	3.60	3.79		
Pt	$\bar{x}$=21.47	19.07	18.67	24.93	19.60	25.36	2.430	.056
	sd= 8.48	6.80	5.60	9.50	9.29	8.98		
Sc	$\bar{x}$=20.82	18.13	17.40	23.93	18.80	26.21	2.273	.070
	sd=10.28	7.67	7.68	12.33	8.74	12.31		
Ma	$\bar{x}$=17.66	17.67	15.73	19.33	17.40	18.21	1.295	.280
	sd= 4.49	4.05	4.43	4.47	4.56	4.77		
Si	$\bar{x}$=35.08	34.27	33.53	36.80	32.93	38.07	1.141	.344
	sd= 7.93	7.03	6.50	9.24	8.66	7.71		
H.duration[2] (in years)	$\bar{x}$= 8.99	12.27	8.73	8.80	8.13	6.69	1.542	.200
	sd= 6.36	5.97	6.75	6.96	6.10	5.28		
H.severity	$\bar{x}$= 7.31	6.87	6.87	7.87	7.27	7.67	0.760	.555
	sd= 2.01	1.85	2.26	2.45	1.58	1.88		
Baseline measures:								
H.index	$\bar{x}$=289.85	211.80	234.64	457.68	279.25	265.89	2.149	.084
	sd=265.35	155.44	247.40	329.62	267.74	256.46		
medication	$\bar{x}$= 3.62	3.37	1.27	3.63	3.97	6.04	1.175	.330
	sd= 5.99	5.75	1.69	7.54	4.12	8.42		
H.days[3] p.w.	$\bar{x}$= 4.30	3.70	3.77	5.38	4.15	4.50	1.764	.146
	sd= 2.04	1.96	2.23	1.98	1.64	2.13		
H.duration p.w. (hours)	$\bar{x}$=43.85	37.92	38.80	63.82	42.16	36.04	1.924	.116
	sd=32.60	31.53	32.56	38.41	30.21	24.22		
H.intensity	$\bar{x}$= 5.59	5.28	5.11	6.62	5.45	5.49	1.345	.262
	sd= 2.00	1.47	2.21	2.18	1.79	2.18		
Expected[4] results	$\bar{x}$= 3.46	3.57	3.67	3.73	3.43	2.86	1.270	.291
	sd= 1.19	1.16	1.18	1.58	0.76	1.03		

1) See note 1, table 4.2.
2) H.= Headache
3) p.w.= per week
4) See note 4, table 4.2.

Fig. 5.1.: Schematic diagram of the various phases of the headache treatment project (Experiment II), including information about patient flow during the study



+D stands for dropouts

5.2.1. THE SINGLE BLIND ASPECT

In experiment I, treatment credibility as it was perceived by the patients was measured at follow-up. The advantage of this is that the patients have actually experienced the treatment they are to evaluate. There is an obvious disadvantage to this procedure too: Because it is unlikely that in every condition there will be an equal amount of dropouts, differential dropout rates may bias the follow-up results. In line with the recommendation of Andrasik & Holroyd (1980) treatment credibility was measured in experiment II at three points during the study: Directly after the first session (before any dropout could have taken place due to treatment variables), directly after the last session, and at follow-up (six weeks after completion of the treatment). The question in which treatment credibility was evaluated was the same as the one used in experiment I (see 4.2.1.): The patient was requested to state how willing she was to recommend the treatment she received to other patients on a seven-point scale.

Table 5.2.: Patient treatment credibility ratings
(1 = most credible; 7 = least credible)

Time	total	relaxation	concentration	df	F	p
post session 1	$\bar{x}$=2.43 sd=1.29 n=58	$\bar{x}$=2.61 sd=1.40 n=28	$\bar{x}$=2.27 sd=1.17 n=30	1,56	1.016	.318
post-treatment	$\bar{x}$=2.18 sd=1.47 n=45	$\bar{x}$=2.38 sd=1.64 n=24	$\bar{x}$=1.95 sd=1.24 n=21	1,43	0.929	.341
follow-up	$\bar{x}$=2.24 sd=1.61 n=45	$\bar{x}$=2.58 sd=1.93 n=24	$\bar{x}$=1.86 sd=1.06 n=21	1,43	2.343	.133

The answers of all patients who had received relaxation therapy (conditions RR + RC) were compared by one-way analysis of variance with those of all patients who had re-

ceived concentration therapy (conditions CC + CR). The results are contained in table 5.2. They show that neither after session 1 nor at post-treatment or follow-up, was there a significant difference between the credibility ratings for the two treatments. At post session 1 an additional check was done: Patients were asked to state what results they expected to obtain from the treatment they received on a seven-point scale (1=totally gone; 7=worse). This question is contained in the post session 1 patient evaluation form (see Appendix O). The question was included, because it was hypothesized that if, on the average, patients found one of the treatments less credible, this should be reflected in their outcome expectancies. A one-way analysis of variance performed on the results showed no significant difference in expectation of improvement for the two treatment modalities, as can be seen in table 5.3.

Table 5.3.: Patient expectancy of improvement at post session 1 (1 = totally gone; 7 = worse)

Therapy	n	$\bar{x}$	sd	df	F	p
relaxation	28+	3.63	1.36	1,55	1.581	.214
concentration	30	3.23	1.01			
total	58	3.42	1.19			

+Of which one missing value

At follow-up an additional check was done: The patient evaluation form at follow-up (see Appendix R) contained a question, in which the patient was asked to indicate how helpful she had perceived the exercises to be on a seven-point scale. This question was formulated as follows:

> Did the exercises contribute positively or negatively to the treatment of your headache?

The rating on the seven-point scale had a range of 1 (=very positively) to 7 (=very negatively). A one-way analysis of variance performed on the results showed no significant dif-

ference between the two therapies in this respect (see table 5.4.).

Table 5.4.: Perceived helpfulness of the exercises at follow-up by the patients (1 = most helpful; 7 = least helpful)

Therapy	n	$\bar{x}$	sd	df	F	p
relaxation	24+	2.65	1.61	1,42	0.377	.542
concentration	21	2.95	1.63			
total	45	2.80	1.61			

+Of which one missing value

The data presented in this section warrant the conclusion that there are no indications that the control treatment (concentration therapy) was rated as less credible, less helpful, or as inspiring less confidence in results than the experimental treatment (relaxation therapy), and that therefore the single blind aspect of the study has been realized.

5.2.2. THE DOUBLE BLIND ASPECT AND THERAPIST BIAS

As in experiment I information about whether therapist blindness had successfully been realized, was obtained through questions in the therapist evaluation form, which was filled out by each therapist after having completed the treatment of his/her last patient (see Appendix S; an English translation is to be found in 4.2.2., page 52).

The answers were checked on positive evidence of therapist suspicion, as well as on positive evidence of therapist acceptance of the rationale given to them at pre-treatment. Again, as in experiment I, no evidence of therapist suspicion was found in the answers, and most of the therapists' comments were nonrelevant to our purpose. The answers did, however, contain positive evidence that the rationale had indeed been accepted. The parts of the therapist statements

giving information of this kind are listed in table 5.5., for each therapist seperately, beginning with the three female therapists (F 1,2,3) and proceeding with the three male therapists (M 1,2,3).

Table 5.5.: Therapist comments concerning the design of the study

Therapist	Comment
F1	"It was often difficult for me to deceive the patients. About the ethical aspects in general: I am negative about the control conditions."
F2	"I think the research should have been done without the control conditions. .. I had problems doing the therapies in the control conditions.. "
F3	"With some patients in the control groups I felt especially uncomfortable in doing the therapy. Even if they have the opportunity to get the right treatment after this study is over, some will have lost motivation to enter a second therapy."
M1	"I am not negative about the use of control groups as used in this experiment. Only with one patient it was difficult for me to justify to myself what I was doing."
M2	"Do not give information about the condition a patient is in, especially in the case of CR and RC." *(sic! F.W.)*
M3	"Dropping out of treatment is especially tragic in the control conditions, because these people do not have a chance to get the right treatment."

An informal talk with each therapist, after completion of the evaluation form – in which the experimenter nondirectively elicited comments from the therapists, notably about the design of the study – confirmed the data reported in table 5.5.

In experiment II a more direct question concerning the issue of therapist blindness and therapist bias was included also: At post session 1 the patient evaluation form contained a question in which they were asked to indicate which results they expected from the therapy on a seven-point scale. The therapists were told that we wanted to investigate which of the two parties involved in therapy (patient

or therapist) was more accurate in predicting results, and that therefore both parties had to make such a prediction, independently from each other. Therefore, at post session 1 the therapist evaluation form also contained such a question. Our real objective, however, was to investigate whether therapist blindness as well as therapist bias had been created by means of the rationale. If the rationale had produced the desired effects, then the therapist should not predict differential improvement for the two therapies (in a factorial analysis of variance Treatment Given should not have a significant influence on therapist prediction of outcome). This would be evidence that the double blind aspect had been realized. Also, if the rationale had produced the desired effects, we would see differential improvement predictions in the positive therapist bias (RR + CC) versus negative therapist bias (RC + CR) conditions: Therapist Bias in a factorial analysis of variance should produce a statistically significant influence.

The question the therapist had to answer in the post session 1 evaluation form for each patient individually, was identical to the one answered by the patient (an answer had to be given on a seven-point scale; 1 = totally gone; 7 = worse). The results are contained in table 5.6., and the factorial analysis of variance (classic experimental approach) in table 5.7.

Table 5.6.: Therapist expectations of improvement post session 1

Condition	n	$\bar{x}$	sd
RR	14	3.29	1.07
RC	14	4.64	0.93
CC	15	4.07	0.96
CR	15	4.87	0.89
Total	58	4.22	1.14

Table 5.7.: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on therapist expectancy of improvement at post session 1

Source of variation	df	mean square	F	p
Main effects	2	10.11	10.354	.000
Treatment Given (TG)	1	3.66	3.743	.058
Therapist Bias (TB)	1	16.57	16.965	.000
TG x TB interaction	1	1.12	1.151	.288
Explained	3	7.12	7.286	.000
Residual	54	0.98		
Total	57	1.30		

These data show that therapists did not have significantly different outcome expectations with respect to the two therapeutic modalities (the influence of Treatment Given on outcome expectancy was not significant). This means that therapist blindness had successfully been implemented and that the double blind aspect of the study had been realized. Furthermore, these data clearly show that therapists did have significantly different outcome expectancies in the expected direction in the positive therapist bias versus the negative therapist bias conditions. This means that therapist bias had been successfully implanted.

5.2.3. THE INFLUENCE OF THERAPIST BIAS

The second question this experiment sets out to answer ("Is double blind research desirable cq. necessary in psychotherapy") is concerned with therapy outcome and the influence of therapist bias on outcome.

In order to be able to eliminate the possible conclusion that differences in outcome values between conditions after treatment might be attributable to selective differences in therapist's activities across conditions, the data of the Therapist Self-Monitoring Forms (see 5.1.3.5.) were submitted to factorial analysis of variance. The influence of

Treatment Given, Therapist Bias, and their interaction was checked. A summary of the results of these analyses is found in Appendix T. There we can see that only 3 of the 96 F-values computed were statistically significant on .05 level, while purely on a chance basis we could have expected 4 or 5 F-values to be significant. Therefore, we conclude that there is no indication that therapist bias or treatment given significantly influenced the variables measured by the Therapist Self-Monitoring Form, and that possible differences in headache indices at post-treatment or follow-up cannot be attributed to such influences.

The headache index was used for measuring outcome, just as in experiment I (see 4.2.3., pp. 54-55). A survey of changes in headache index in the four treatment conditions compared to change in the waiting list control is contained in table 5.8.

Table 5.8.: Headache indices for experiment II

Condition	Baseline	Post-treatment	Follow-up
RR	$\bar{x}$= 224.00	$\bar{x}$= 207.22	$\bar{x}$= 221.36
n=13	sd= 163.48	sd= 281.68	sd= 277.45
RC	$\bar{x}$= 263.20	$\bar{x}$= 198.35	$\bar{x}$= 169.14
n=11	sd= 279.47	sd= 269.08	sd= 324.61
CC	$\bar{x}$= 458.02	$\bar{x}$= 384.00	$\bar{x}$= 274.41
n=11	sd= 355.63	sd= 302.32	sd= 314.65
CR	$\bar{x}$= 232.80	$\bar{x}$= 201.21	$\bar{x}$= 225.64
n=10	sd= 265.94	sd= 292.63	sd= 343.79
W	$\bar{x}$= 276.21	$\bar{x}$= 264.74	
n=12	sd= 274.47	sd= 283.65	

The one-way analysis of variance of the baseline data in table 5.1. was computed for all patients, including the dropouts. The figures in table 5.8. are based only on pa-

tients who remained in therapy. Therefore, a one-way analysis of variance was performed again on the baseline values of the five experimental groups, to check whether they were still equivalent after dropouts had been deleted. No significant overall differences were found (df=4,52, F= 1.391, p=.2499). However, the Multiple Range test in which a pairwise comparison was done, established that a significant difference in baseline level between the CC and RR groups was present at the .05 level. The substantial differences between the groups on baseline levels which occurred despite random assignment to conditions, may obscure a clear assessment of treatment effects. Therefore, it was decided to employ analysis of covariance (with baseline as covariate) for subsequent analyses on post-treatment and follow-up headache index data, as was also done in experiment I.

Then the data analysis focused on establishing whether any differences between the five groups at post-treatment existed. This was done by analysis of covariance (with baseline as covariate and groups as main effect). The results contained in table 5.9. show that there was no significant difference between the five groups. A pairwise comparison of each group individually with the waiting list control yielded the same results: No treatment group was

Table 5.9.: Differences in headache indices between the five conditions at post-treatment

Source of variation	df	mean square	F	p
covariates baseline	1	3405610.00	158.882	.000
main effects condition	4	6034.75	0.282	.889
explained	5	685949.75	32.002	.000
residual	51	21434.82		
total	56	80766.50		

Table 5.10.: Headache index at post-treatment: A pairwise comparison of the treatment groups with the waiting list control (corrected for baseline differences)

Comparison	df	F	p
RR vs. W	1,22	0.002	.964
RC vs. W	1,20	2.491	.130
CC vs. W	1,20	0.063	.804
CR vs. W	1,19	0.320	.578

significantly different from the waiting list control at post-treatment (see table 5.10.). In other words, no treatment condition was significantly better than the waiting list control.

A factorial analysis of covariance (2 x 2 design, with baseline as covariate), investigating the effects of Treatment Given (relaxation or concentration therapy) and Therapist Bias (positive, RR + CC, and negative, RC + CR), found no significant influence of any of these variables on outcome, neither at post-treatment (see table 5.11.), nor at follow-up (see table 5.12.).

Table 5.11.: Analysis of covariance for the effects of Treatment Given (2) x Therapist Bias (2) on post-treatment headache index (with baseline headache index as covariate)

Source of variation	df	mean square	F	p
Within + residual	37	20704.17		
Constant	1	2743824.08	132.525	.000
Baseline within TG x TB	4	686542.86	33.160	.000
Treatment Given (TG)	1	48833.04	2.359	.133
Therapist Bias (TB)	1	3176.29	0.153	.698
TG x TB	1	70643.75	3.412	.073

Table 5.12.: Analysis of covariance for the effects of Treatment Given (2) x Therapist Bias (2) on follow-up headache index (with baseline as covariate)

Source of variation	df	mean square	F	p
Within + residual	36+	18473.71		
Constant	1	2179031.42	117.953	.000
Baseline within TG x TB	4	834306.67	45.162	.000
Treatment Given (TG)	1	10879.89	0.589	.448
Therapist Bias (TB)	1	1834.48	0.099	.754
TG x TB	1	326.30	0.018	.895

+One missing value

As in experiment I, the same issue was also addressed by using change scores instead of analysis of covariance (for the rationale of this procedure, see 4.2.3., pp. 54-56). Tables 5.13. and 5.14. contain the change scores (computed on the basis of the headache indices) for the various conditions at post-treatment (table 5.13.) and at follow-up (table 5.14.).

In order to investigate the effects of the treatment given the data for the RR and RC conditions were collapsed into R (relaxation) and the data for the CC and CR conditions into C (concentration). The post-treatment scores of R, C, and W were significantly different (L-test: L=10.310, df=4, $p<.05$). A pairwise comparison (see table 5.13.) yielded the following results: Relaxation therapy was significantly superior to the waiting list control group (L=10.008, df=2, $p<.05$), but not to the concentration therapy (L=1.902, df=2, n.s.), while concentration therapy was not significantly different from the waiting list control (L=5.072, df=2, $p>.05<.10$).

At follow-up the difference between relaxation therapy and concentration therapy remained nonsignificant (L=4.948, df=2, $p>.05<.10$), even when dropouts were included in the

Table 5.13.: Post-treatment change scores

Condition	n	much improved (>50%)	slightly improved (20-49%)	unimproved or worse (<20%)
RR	13	6	3	4
RC	11	4	1	6
CC	11	3	3	5
CR	10	2	3	5
W	12	0	4	8
Relaxation (RR+RC)	24	10	4	10
Concentration (CC+CR)	21	5	6	10
Positive TB+ (RR+CC)	24	9	6	9
Negative TB (RC+CR)	21	6	4	11

+TB stands for Therapist Bias

Table 5.14.: Follow-up change scores

Condition	n	much improved (>50%)	slightly improved (20-49%)	unimproved or worse (<20%)
RR	13+	5	1	6
RC	11	7	1	3
CC	11	3	5	3
CR	10	3	2	5
Relaxation (RR+RC)	24+	12	2	9
Concentration (CC+CR)	21	6	7	8
Positive TB++ (RR+CC)	24+	8	6	9
Negative TB (RC+CR)	21	10	3	8

+One missing value

++TB stands for Therapist Bias

analysis and classified as having improved less than 20% ($L=5.356$, $df=2$, $p>.05<.10$). This means that although relaxation therapy produced a larger proportion of patients who improved to a clinically significant extent, this failed to reach statistical significance.

The effects of therapist bias were investigated by collapsing the RR and CC conditions into one category (positive therapist bias), and also the RC and CR conditions into one category (negative therapist bias). No significant differences were found between the two categories, neither at post-treatment ($L=1.008$, $df=2$, n.s.); nor at follow-up ($L=1.212$, $df=2$, n.s.). Even when dropouts were included in the analysis, being classified as having improved less than 20%, these results were maintained; at post-treatment ($L=1.766$, $df=2$, n.s.), as well as at follow-up ($L=1.358$, $df=2$, n.s.).

Reviewing the headache index data of the analysis of covariance as well as the change scores approach, the conclusion is inescapable that Therapist Bias has no significant effect on outcome both at post-treatment and at follow-up, and that for Treatment Given the evidence is contradictory: analysis of covariance showed that R, C, and W were not significantly different from each other, while the change scores approach established that at post-treatment relaxation was significantly different from the waiting list control, but not from concentration therapy. In contrast to the results of experiment I, these results do not clearly favor relaxation therapy.

Let us compare these results to those obtained by (a) global therapist judgement of improvement, and (b) global patient judgement of improvement. The post-treatment therapist evaluation form (see Appendix P) contained a question in which the therapist had to estimate how much the patient had improved. This form was filled out each time after a patient had completed treatment. The estimation had to be given on a seven-point scale (1 = totally gone; 7 = worse). The results are contained in table 5.15.

Table 5.15.: Global therapist judgement of improvement at post-treatment

Condition	n	$\bar{x}$	sd
RR	13	3.62	1.80
RC	11	4.18	1.08
CC	11	3.55	1.21
CR	10	4.80	1.23
Total	45	4.00	1.43

Table 5.16.: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on global therapist ratings of outcome at post-treatment

Source of variation	df	mean square	F	p
Main effects	2	4.82	2.500	.095
Treatment Given (TG)	1	0.71	0.369	.547
Therapist Bias (TB)	1	8.84	4.583	.038
TG x TB interaction	1	1.32	0.685	.413
Explained	3	3.65	1.895	.146
Residual	41	1.93		
Total	44	2.05		

A factorial analysis of variance (classic experimental approach) was performed to investigate the effects of Treatment Given and Therapist Bias. This analysis is contained in table 5.16. The results show that only Therapist Bias had a significant effect on therapist outcome ratings. Neither Treatment Given nor the interaction of TG and TB produced significant results. These findings imply that the therapists' judgement was biased: On the average they reported a greater amount of improvement in the conditions in which they believed that the patients were receiving the appropriate therapy (see table 5.15.) and this difference was significant (see table 5.16.). Also, in line with the

bias implanted in the therapists, they reported no overall difference in the effectiveness of relaxation therapy and concentration therapy.

At post-treatment as well as at follow-up patients gave their judgement of how much they had improved during therapy using the same seven-point scale as the therapists. The results are contained in table 5.17., while the factorial analyses of variance are contained in table 5.18.

Table 5.17.: Global patient judgement of improvement at post-treatment and at follow-up

Condition	n	post-treatment		follow-up	
		$\bar{x}$	sd	$\bar{x}$	sd
RR	13	4.23	1.48	4.38	1.80
RC	11	3.64	1.43	3.18	1.33
CC	11	4.18	1.54	3.82	1.72
CR	10	3.90	1.45	4.30	1.34
Total	45	4.00	1.46	3.93	1.60

Table 5.18.: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on global patient judgement of outcome at post-treatment and at follow-up

Source of variation	df	post-treatment			follow-up		
		mean square	F	p	mean square	F	p
Main effects	2	1.17	0.536	.589	1.22	0.488	.617
Treatment Given (TG)	1	0.11	0.049	.827	0.55	0.220	.641
Therapist Bias (TB)	1	2.25	1.031	.316	1.93	0.770	.385
TG x TB interaction	1	0.27	0.125	.726	7.91	3.166	.083
Explained	3	0.87	0.399	.754	3.45	1.301	.262
Residual	41	2.18			2.50		
Total	44	2.09			2.56		

The factorial analysis of variance (classic experimental approach) showed that, both at post-treatment and at follow-up, neither Treatment Given nor Therapist Bias or their interaction, produced any significant effects. This means that on the average patients reported an equal amount of improvement in the relaxation and concentration therapies, and that therapist bias did not influence their judgements.

5.3. DISCUSSION AND CONCLUSIONS

As in experiment I three outcome parameters were used: Headache index scores, global therapist judgement of improvement and global patient judgement of improvement.

The finding of experiment I that, as far as the headache index is concerned, relaxation therapy was consistently superior to concentration therapy, was not replicated in this study: Although on change scores relaxation therapy was found to be superior to the waiting list control at post-treatment (while concentration therapy was not), relaxation therapy was not significantly better than concentration therapy. Furthermore, in the factorial analysis of covariance, Treatment Given as main effect did not produce significant results, an additional indication that in experiment II there was no significant difference in effectiveness between relaxation therapy and concentration therapy.

The finding of experiment I that the therapists, in line with the bias implanted at pre-treatment, judged the two treatments as being on the average equally effective, but significantly less effective when applied to the "wrong" type of patient, however, was replicated: Treatment Given in the factorial analysis of variance did not produce a significant effect, while Therapist Bias did.

The finding of experiment I showing that patients rated both therapies as being equally effective and that therapist bias did not significantly influence these results,

was replicated in this experiment too. Furthermore, the results of paragraphs 5.2.1. and 5.2.2. are a successful replication of the findings reported in 4.2.1. and 4.2.2.: Both patients and therapists rated the two treatments used as being equally credible in experiment II too.

Summarizing the results of experiment II we can conclude that this experiment replicated the findings that (a) double blind research is feasible in psychotherapy (see 5.2.1. and 5.2.2.), and that (b) when the therapist is the one who judges outcome, it is necessary to use the double blind design (see 5.2.3.).

6.1. SUMMARY

The theoretical part of this study departed from the rather disturbing fact that, despite more than forty years of intensive research, psychotherapy has known no real breakthroughs. Many different psychotherapeutic systems which all claim to be effective in treating the neurotic disorders exist – and the number is still growing.

The majority of the current systems of psychotherapy have produced little or no research to substantiate their therapeutic claims. Still, a considerable number of the psychotherapeutic modalities did produce outcome research. However, this research has not been able to produce many new insights beyond the conclusion that, on the average, all therapies appear to be modestly but equally effective, and that apparently the specific techniques contribute little to improving therapeutic outcome.

This situation was found to be remarkably similar to the state that medicine was in at the turn of the century. A review of the literature showed that during the first half of the present century, medicine managed to emancipate itself from this situation. Controlled research, culminating via the single blind design in the double blind design, provided the tools for this therapeutic breakthrough. In single blind research the patient does not know if he is

receiving the experimental or the control treatment. This manipulation controls for patient bias. In double blind research, patient as well as therapist are blind as to who is receiving the control treatment and who is receiving the experimental treatment. This way both patient and therapist bias are controlled for. In pharmacotherapeutic research this is accomplished by using "identical matching placebos".

A review of the psychotherapeutic outcome research literature showed that until quite recently research strategies in psychotherapy had remained largely on the level of uncontrolled clinical treatment reports. Controlled research of the single blind variety has only been executed to any appreciable extent since the 1960s, and almost exclusively in the area of behavior therapy.

The double blind design, now generally considered to be a "conditio sine qua non" for adequate pharmacotherapeutic research, has been proclaimed to be impossible for research in psychotherapy. In the present study it was shown that this conclusion was premature: An alternative method for inducing therapist blindness (which is the crucial feature of the double blind design) that is workable in psychotherapeutic research, was proposed.

In the empirical section of this study this alternative method to produce therapist blindness was implemented: Two therapies were employed in both experiments. The experimental treatment was relaxation therapy and the control treatment was concentration therapy. The control treatment was devised by the experimenter in such a fashion that, procedurally it was as similar as possible to the experimental treatment, but did not contain the theoretically critical ingredients. The two treatments were presented to both therapists and patients as being bonafide therapies of equal standing. This way therapist bias was distributed equally across the two treatments. Furthermore, in order to investigate the influence of therapist bias, both therapies were administered under negative as well as

positive therapist bias.In both experiments tension headache was the target symptom. Patients were females in the age range of about 18-45. Experiment II was in essence a more sophisticated replication of experiment I.

The major conclusions reached were that (a) double blind research is indeed feasible in psychotherapy, and (b) that, at least in the common situation in which the therapist is also the person who judges outcome, it is necessary to use the double blind design in order to avoid spurious results.

6.2. SOME REFLECTIONS ON THE RESULTS

6.2.1. THERAPIST BIAS

One of the findings of the two experiments was that therapist bias only influenced outcome when this was measured by the therapist himself: The therapist reported significantly more improvement for the patients in the positive therapist bias conditions than for patients in the negative therapist bias conditions. No such differences were found on global patient ratings and headache index values.

In chapter 2 (p. 14) it was noted that therapist bias may result (a) in overrating of improvement by the therapist in the experimental group compared to the control group, and (b) in differences in therapist behavior in the two conditions, leading to the inadvertent communication of his prognostic expectations to the patients and consequently to a direct influence on the amount of improvement the patient reports or even on the symptom itself.

Therefore, it may be concluded that in the two experiments apparently only (a) has occurred. Indications that (b) did not take place are the following: In experiment II the results of the Therapist Self-Monitoring forms showed that therapists did not transgress more from the therapy-"script" in the positive therapist bias groups than in the negative therapist bias groups. Even the amount of time they

spent on the various parts of each session did not differ significantly across conditions. This seems to imply that apparently a clear-cut "therapy-script" for each session, together with therapist self monitoring effectively prohibited the communication of the therapist's prognostic expectations to the patients. This hypothesis is supported by the fact that in a factorial analysis of variance neither patient expectations of results (see Appendix U) nor patient evaluation of the credibility of the two therapies (see Appendix V) showed a significant effect of therapist bias. However, it may still be that in cases where the therapist has more freedom in varying the content of the session, communication of prognostic expectancies has more chance of occurring; and that in these cases the therapist bias may also influence patient reports of improvement and possibly "hard" outcome measures as well.

6.2.2. THE "MAGICAL" TWO-THIRDS IMPROVEMENT RATE

With respect to the effectiveness of psychotherapeutic methods it has been noted that "..the figure of 2/3 *(of the patients improving considerably)* emerges again and again in a remarkable constancy, irrespective of what form of therapy was undertaken .. especially when the data are pooled." (Nawas, Pluk & Wojciechowski, 1980: 8). Apparently the psychotherapeutic modalities are equally effective, or as Luborsky, Singer and Luborsky (1975) stated: "Everybody has won, and all must have prizes".

However, there is an important exception to this "magical" two-thirds rate of improvement which has escaped attention until now: The placebo control therapies as used in (single blind) behavior therapy research have often produced improvement rates that are well below those of "real" psychotherapies, especially in the early years, when strong beliefs were held by behavior therapists about the efficacy of their own methods. In their meta-analysis of comparative

outcome studies, Shapiro & Shapiro (1982:596) concluded, that "active treatments were superior to minimal, placebo treatments in the majority of such comparisons".

Still these control treatments are no less weird or incredible than a host of other, "real" therapies, such as Primal Scream Therapy, Rebirthing, or for that matter, Yoga. The main difference between "placebo" control treatments and those therapies, however, is that in the cases of the control treatments the therapist "knows" that he is administering a "fake" treatment, while he considers these other treatments as being bonafide therapies.

As we have discussed in chapter 2 and in 6.2.1. this therapist "knowledge" constitutes a bias that may well have a significant effect on the results, and that to a large degree this therapist bias may be responsible for the differences found. This implies that as the facts stand now, we cannot (as yet?) conclude that psychotherapy is better than (equally credible) control treatments.

Another point that deserves attention is the following: How real is this figure of two-thirds? Can it be that it is just an artifact produced by the crude and bias-prone way of measuring outcome that still prevails in psychotherapeutic research? The two experiments described in this study provide some evidence that this indeed may be the case: When we consider the headache change scores of the two experiments (see tables 4.11. and 5.14.) at follow-up[1], we see that in experiment I, eighteen of the 36 patients who had completed treatment had a considerable reduction ($\geq$50%) in headache activity, this is 50% of the patients. In experiment II, eighteen of the 44 patients (=41%) had an equivalent reduction in headache activity[2]. Pooling of the

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1) 45 Patients actually completed therapy in experiment II, but of one patient headache index data were missing for the follow-up period.

2) Only for the follow-up data global patient judgements of improvement and headache change scores are available for both experiments, making pooling of the data possible.

data for the two experiments, we find that 36 of the 80 patients (=45%) have at least 50% reduction in headache activity. This is well below the magical two-thirds. However, the headache index is a relatively "hard" measure of outcome, rarely used in psychotherapeutic research.

Let us see what figures turn up when we use the more common global reports of improvement (see Appendix W): In experiment I, 27 of the 36 patients reported a reduction of at least 50% in headache activity at follow-up, this is 75% of the total. In experiment II, 27 of the 45 patients who completed treatment reported at least 50% reduction. This is 60%. Pooling the data we find that 54 of the 81 patients reported at least fifty percent reduction in headache activity: this is 66.67%, which is exactly the magical two-thirds! (I would gladly have settled for a percent more or less, because this result is so perfectly 2/3, that it smacks of being "to good to be true", but this is what I have found, and this is what I have to report.) The fact that the 2/3 figure was only found when using global patient judgements, and not when using a relatively "hard" outcome measure (headache index data) strongly suggests that this so often encountered 2/3 rate of improvement may be artifactual in nature, a result of the method of measuring outcome.

6.2.3. THERAPEUTIC EFFECTIVENESS

One finding of experiment I that was not replicated in experiment II was, that relaxation therapy was the superior treatment. In experiment II neither relaxation nor concentration therapy was consistently superior to the waiting list control group. How can this be explained, given the fact that the two experiments were identical in most aspects? The experimental design was identical, the same treatments were used, the target symptom was tension headache in both experiments, and in both experiments graduate students of psychology were used in the same sex ratio

(3 males and 3 females in each of the experiments).

There were some divergences however, that may be causally related to the differences in outcome: In experiment II both patients and therapists had to fill out quite a number of evaluation forms and questionnaires during the course of therapy. This may have stressed the research aspects of the study in the eyes of the patients and have lessened their "being in therapy" feeling that is so crucial for making therapeutic change possible (Nawas, Pluk & Wojciechowski, 1980). Furthermore, the filling out of the Barrett-Lennard Relationship Inventory at post session 1 in experiment II proved to be highly aversive to a good many patients: Quite a few objected to having to fill out this form, because they thought it impossible to give their opinion about the therapist and about how they expected the therapeutic relationship to develop so early in therapy. After a discussion of the rationale for the inclusion of this form all patients, with the exception of one, did fill out the form conveying in the answers, as instructed, how they expected the relationship to develop on basis of the few impressions obtained during the first session. Nevertheless, this filling out the Barrett-Lennard had proved to be a negative experience for many patients, in a very early stage of therapy. In experiment I, neither patients nor therapists were required to fill out forms during therapy.

A further consideration is the following: In experiment II after completion of each session the therapists had to fill out a self-monitoring form, which encouraged them to adhere as closely as possible to the therapy-"script"; in experiment I they had and more often also took more liberty in departing from the "script" when they deemed this desirable. This restricted liberty in adapting therapy to individual patients' needs may also have contributed to lessening the effectiveness of both therapies in experiment II. The above may be construed as an empirical support to Schaap's (1982) impression that methodologically "ideal"

psychotherapeutic effect research and clinically "ideal" psychotherapy cannot take place at the same time.

6.3. SOME REFLECTIONS ON THE RESEARCH METHODOLOGY

6.3.1. ETHICAL ASPECTS

In the two experiments described in this study, a control treatment was used. Some patients were assigned to a therapy (concentration therapy), that is without theoretical rationale as far as tension headache is concerned. It might be argued that it is unethical to withold the supposedly active treatment (relaxation therapy) from these patients. The crux of the matter, however, lies in the word "supposedly", because it is only through controlled research that we may arrive at solid knowledge about the effectiveness of treatments. Without such controlled research we can never be certain that the "supposedly" active treatment is indeed superior in effectiveness. As Foulds already concluded in 1958: "The witholding of treatment X cannot be unethical if it is not known to be efficacious. Continuing to give treatments with no intention of validating them would seem to be the more immoral course." (Foulds, 1958:261).

Sleisenger, totally independent of Foulds, came to the same conclusion: Using the double blind method to evaluate new treatments "..is much more than defensible, for it is well to remember that years of unscientific evaluation of pharmacological agents not only retarded the scientific development of clinical medicine, but also contributed to a gigantic waste of time, effort, and hope on the part of the countless patients seeking help." (Sleisenger, 1958:416).

The above and similar arguments led to the general acceptance of the double blind design in pharmacotherapeutic research, and convinced me that controlled research of the double blind variety is not unethical in the field of psychotherapy either, provided that the following conditions

are met: The control treatment used should not have harmful effects on the patient. It should contain as many of the theoretically noncritical therapeutic ingredients as possible. Furthermore, the patients in the control treatment who have not experienced a satisfactory reduction in headache symptomatology should be provided with the opportunity of receiving additional treatment that, on theoretical grounds, is considered as being more appropriate. In the two experiments described in this study this was accomplished as follows: Concentration therapy was used as the control treatment. There was no theoretical or empirical evidence that concentration therapy was potentially harmful to patients, while the treatment did contain a good many "nonspecific" therapeutical ingredients such as daily self-monitoring of symptoms, daily exercises, an opportunity to discuss stressful life events during the verbal part of the therapy, etc. Furthermore, after completion of the follow-up period all patients in the control conditions who indicated that they had obtained a less than satisfactory reduction in headache activity, were offered additional treatment by the experimenter or one of his colleagues, or – if patient background suggested that a kind of therapy was indicated that was not available at the Department of Clinical Psychology – patients were given advice as to where to go for their complaint.

6.3.2. STUDENT THERAPISTS

Paul's famous study (Paul, 1966) has been critized for not having used real patients, but student volunteers instead. Our study might evoke the criticism that no "real" therapists have been used and that therefore generalization to "real" psychotherapeutic research is problematic. In our opinion this is an unwarranted criticism: The student therapists were graduate students of psychology and because their therapeutic tasks were rather limited (they had to

treat patients with only one complaint, and with either relaxation or concentration therapy) their therapeutic training could remain limited too. Furthermore, as Durlack (1979) has concluded in a careful review, there is no convincing evidence that paraprofessionals with limited on the job training produce inferior results compared to experienced professionals in psychotherapy. Finally, research addressing itself specifically to the influence of therapist experience on the effectiveness of behavioral treatment of tension headache (Blanchard, Andrasik, Neff, Saunders et al, 1983), using therapists of varying levels of experience, found that the experience level of the therapists had no significant effect on outcome. Therefore, they concluded that their results were "..consistent with the position that short-term intensive training in relaxation and biofeedback techniques is sufficient to ensure patient improvement, and that more extensive experience does not necessarily result in improved outcome." (Blanchard et al, 1983:212).

Another possible criticism is that the therapists employed in the two experiments, due to their lack of extensive experience, are more manipulable by the experimenter than experienced therapists are. Therapist bias may then be a problem only when using such inexperienced, highly manipulable therapists. Research addressing this question directly has not been found in the literature. However, one study was discovered that closely approximates it: In an empirical study Temerlin (1968) explored the effect of interpersonal influences on psychiatric diagnosis as made by psychiatrists (n=25), clinical psychologists (n=25), and graduate students of clinical psychology (n=45). All were exposed to a sound-recorded interview with a normal, healthy man. Just before listening to the interview, they heard a professional person of high prestige who was acting as a confederate of the experimenter say that the individual to be diagnosed was "a very interesting man because he looked quite neurotic but actually was quite psychotic". Three matched control

groups, stratified on professional identity, also listened to the interview, but did not receive the suggestion of psychosis being present. The results were startling: None of the subjects in the control conditions ever diagnosed psychosis, while in the high prestige suggestion condition 60% of the psychiatrists, 28% of the clinical psychologists, and 11% of the graduate students of psychology did. Furthermore, the graduate students most often diagnosed the "patient" as being healthy or having pathology less severe than psychosis, and the psychiatrists did this least often. (None of the 25 psychiatrists even diagnosed the "patient" as being healthy!) If anything, these findings do not indicate that graduate students of clinical psychology are more likely to be influenced by experimenter suggestions than more experienced professionals.

6.3.3. MONO-SYMPTOMATIC TARGET COMPLAINT

In the two experiments described in the empirical section of this study, a rather homogeneous sample of patients was employed. Only patients with tension headache (or mixed migraine-tension headache) were accepted for treatment. Furthermore, participants were limited to females between the ages of 18 and 45. This may evoke the criticism that this study is of limited relevance for the bulk of psychotherapeutic research, because such homogeneous groups are rarely used in psychotherapeutic research.

However, as stated in the introduction, the most important question in psychotherapy is the one posed by Paul (1967: 111): "What treatment, by whom, is most effective for this individual with that specific problem, and under which set of circumstances?". If we want to obtain answers to this question, we cannot afford to continue doing outcome research with undefined samples, target symptoms and therapies. The systematic variation of one or a few elements in Paul's question and controlling for the others, is then the only pos-

sible method of arriving at a more solid basis for psychotherapy.

Such procedures are necessary in order not to relapse into the situation of the early sixties when the disappointing results of the clinical treatment reports with their heterogeneous patient groups, led to a flight from outcome into process studies: As long as the effectiveness issue is not yet settled this is a dead-end alley, that only can produce data about what happens in a procedure about which it is not even known whether it works. In our experiments therapist bias and treatment given were varied systematically, both variables having two levels, while the other variables were controlled for.

Nevertheless, it may justly be said that, because of the homogeneity of the sample, and the standardized application of the treatments, conclusions about the efficacy of relaxation therapy per se in the treatment of tension headache are not warranted. We have to keep in mind, however, that this was not the objective of the present study. The two experiments were not intended to establish the efficacy of relaxation therapy, but to study the effects of therapist bias and to show that these effects can be controlled for.

6.4. RECOMMENDATIONS FOR FUTURE RESEARCH

The two experiments described in this work, clearly established that double blind research is feasible in psychotherapy and that it is certainly necessary when the therapist is the judge of outcome.

In the two experiments we induced the double blind condition by devising a control condition that was procedurally similar to the experimental condition and by presenting it to both therapists and patients as being a bonafide therapy. When this method becomes more widely known in psychotherapeutic circles, this particular procedure for inducing the double blind will of course become increasingly prob-

lematic: Therapists will have read or heard about this method and be suspicious of the rationale presented to them. It is likely that they will assume that the therapy that is unknown to them is the control therapy, in spite of the information in the rationale.

In behavior therapy research, this problem can largely be circumvented. Due to the technique oriented approach of a large part of behavior therapy, it is in behavior therapy – to a larger extent than in other psychotherapies – feasible to use paraprofessional therapists with a relatively brief training in the treatment modalities that will be investigated. For this population of therapists it will remain relatively easy to induce the double blind in the aforementioned manner. For therapies in which professional therapists will be employed, new methods of creating the double blind will have to be developed.

In order to illustrate the feasibility of other methods, let us return to the essential feature of the double blind: The therapist does not know when he is giving the "real" treatment and when he is giving the "control" treatment. In a large number of cases this may also be accomplished by simply not informing the therapist of the experimental hypothesis. For instance, when the researcher has reason to believe that, say, therapy X works better for introverts and therapy Y better for extroverts, then the same design as the one used in the two experiments described in this study may be used for these two (bonafide) therapies: A factorial design in which one half of the introverts get therapy X and the other half therapy Y (and the same is done for the extroverts). In all conditions then the therapist only knows that he is administering a bonafide therapy, and therapist bias thus will not be distributed unevenly across conditions.

However, in a number of cases double blind research still remains impossible. This is especially the case in long-term outcome research, where it is obviously unethical to

continue witholding the patients in the control conditions the experimental treatment, if in the course of the study the value of the experimental treatment becomes increasingly clear.

In pharmacotherapeutic research there is an additional problem which has given rise to a solution that is also relevant to psychotherapeutic research: A number of drugs have quite obvious side effects, which placebos have not, thus breaking therapist blindness already early during the study. In 1967 Guy, Gross & Dennis had already pointed to "an alternative to the double blind procedure" in such cases: The use of an Independent Assessment Team (IAT): "Where it is not possible to maintain 'blindness' of the therapist, the IAT as well as the patient can remain 'blind', simulating closely the classical situation" (Guy et al, 1967:1510). They further point out that even when the IAT itself inadvertently breaks the "blind" conditions (e.g. when the patient reveals the nature of his treatment during assessment procedures) the lack of direct involvement in the treatment process tends to reduce therapist bias.

The results of our two experiments suggest another source of bias which is not covered by either the double blind method per se or the alternative of Guy et al.: The value of global post hoc outcome judgements by either patient or therapist has been greatly questioned. Therapists tend to report improvement rates in line with the bias implanted in them. Furthermore, patients as well as therapists tend, on the average, to report an equal amount of improvement for the two treatments under investigation, as long as they perceive the two as being of equal standing. The desirability of devising more "hard" measures of outcome, independent of subjective post hoc judgements by participants, has been stressed. Examples of such alternative assessment possibilities are: daily patient self-monitoring data, physiological measures (where possible), observational data, and "criterion oriented" therapy (this means that before

the beginning of therapy the therapeutic goals are unambiguously operationalized so that the meeting of these criteria at post-treatment can be measured objectively). The results of our experiments even suggest, that when such hard measures are used, and therapists have clear and detailed guidelines for doing the therapies, such hard measures alone may be a viable alternative to the double blind procedure.

A final consideration concerns therapist bias as a socio-psychological phenomenon. In the present study the focus was on: "Does therapist bias influence psychotherapeutic outcome" and "how can therapist bias be controlled for in psychotherapeutic outcome studies". Therefore, the objectives of this study were primarily methodological in nature. The phenomenon of therapist bias also has a socio-psychological aspect: "How does therapist bias come about" and "how does it influence outcome". These questions are akin to those Rosenthal (1966) investigated in his experimenter bias experiments. It seems worthwhile to study these "process" and socio-psychological aspects of therapist bias too, and it is recommended that such research be undertaken now that we know that therapist bias does influence outcome in psychotherapeutic research.

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Appendix A

ACHTERGRONDEN EN DOELSTELLINGEN

De bedoeling van dit onderzoek is een bijdrage te leveren aan het specifiek maken van de zgn. nonspecifieke factoren in de psychotherapie, zodat bij indicatiestelling een verantwoorde keuze gemaakt kan worden voor een bepaalde therapie, gedaan door een bepaalde therapeut, bij een bepaald type patiënt met een bepaald type probleem.
Het beantwoorden van al deze vragen tegelijkertijd maakt enorm grote patiënten- en therapeuten aantallen nodig. Daarom worden door o.a. randomisering en 'matching' een aantal variabelen onder controle gehouden en slechts een beperkt aantal onder de loupe genomen.
We nemen alleen vrouwelijke patiënten tussen 18 en 45 jaar met spanningshoofdpijn of 'mixed' migraine-spanningshoofdpijn (zuivere migraine sluiten wij uit omdat daarbij additionele therapeutische maatregelen nodig zijn). De therapeuten zijn geselecteerd op homogeniteit. Wij hebben evenveel mannelijke als vrouwelijke therapeuten genomen, etc., etc.
Wat we gaan doen en waarom volgt nu:
De cognitieve theoreticus en 'fixed role' therapeut George Kelly deelt de mensen in aan de hand van een dimensie, die varieert van cognitief 'complex' tot cognitief 'simplex'.
Cognitief 'complexe' mensen hebben zeer gedifferentieerde cognitieve schema's en percepties, waarin een grote hoeveelheid details (waarvan vele, in verband met de zaak die aandacht vraagt, irrelevant zijn) waargenomen worden en de aandacht krijgen.
Cognitief 'simplexe' mensen daarentegen hebben een gebrekkig vermogen om details en nuances waar te nemen. Zij zijn zeer globaal in hun perceptie van zichzelf en de omringende wereld.
Bijna equivalente termen voor de 'complex' - 'simplex' dimensie zijn 'sharpeners' en 'levelers'. Holzman & Klein gebruiken deze termen om ongeveer dezelfde dimensie ('complexity' - 'simplicity') te beschrijven.
Iedereen heeft dagelijks veel taken die hij verrichten moet, of hij nu extreem 'simplex' of 'complex' is, of een middenpositie op deze dimensie inneemt. Degenen die extreme posities op deze dimensie innemen, hebben bijzondere problemen met het naar behoren functioneren in het dagelijkse

leven, juist vanwege hun perceptueel-cognitieve 'style' (levensstijl). De zeer gedifferentieerde 'complex-sharpener' neemt a.h.w. te veel in zich op en raakt verstrikt in te veel details. Dit interfereert natuurlijk met zijn functioneren. Wanneer men niet in staat is de taken die men moet verrichten te vervullen zoals men wil, dan wordt men gespannen, angstig, etc. Deze spanning en angst reduceren op hun beurt weer de efficiëntie en leiden tot een hyper-alertie voor stimuli (analoog aan de 'fight-flight' reactie en de ermee gepaard gaande autonome veranderingen onder stress). Het gevolg is een vicieuze cirkel, die verbroken dient te worden. 'Relaxation training' is de beste methode gebleken om deze vicieuze cirkel te doorbreken. De 'style' van iemand kunnen we niet grondig veranderen. Wel kunnen we iemand helpen om met obstakels zoals angst en spanning klaar te komen.

De problemen van de globale 'simplex-levelers' liggen op een ander vlak: Hun probleem is dat ze gedetailleerde, gedifferentieerde nuances onvoldoende waarnemen en verwerken. En het leven is er vol van. Terwijl de karakteristieke reactie van het eerstgenoemde type angst en spanning is, is de voornaamste reactiewijze van de 'simplex-levelers' op hun handicap een gevoel van hulpeloosheid, insufficiëntiegevoelens en rusteloosheid. Ook hier kunnen we de 'style' van deze mensen niet radicaal veranderen, maar we kunnen ze wel manieren leren waarop ze een rijkere variëteit stimuli kunnen verwerken en beter kunnen differentiëren, waardoor ze beter nuances leren waar te nemen. Zulke mensen blijken goed geholpen te kunnen worden met gestructureerde 'in imagine' leerervaringen, 'focussing' (de methode van Gendlin) en het uitvoeren van 'in imagine' concentratieoefeningen, waarin ze de opdracht krijgen hun aandacht op één aspekt te richten en te houden, waarna overgegaan wordt op andere en grotere aantallen details.

Beide therapiemodaliteiten zijn verwant aan autogene training en Yoga technieken . 'Relaxation training' is geënt op de meer lichaamsgerichte technieken en heeft voornamelijk een perifeer aangrijpingspunt. 'Focal concentration imagery' is geënt op de rechtstreeks centraal aangrijpende meditatie- en concentratietechnieken uit Yoga en autogene training.

Het 'Headache Research Project' van de universiteit van Missouri (U.S.A.) heeft in 1978 twee feiten aan het licht gebracht, die voor ons onderzoek van belang zijn:

Ten eerste: de meeste spanningshoofdpijnlijders behoren ofwel tot het ge-

differentieerde-complex-sharpening type of tot het tegenovergestelde, het globale-simplex-leveling type. Om het toekennen van een waardeoordeel te vermijden, noemden de leiders van het project de twee types resp. alpha (complex-type) en beta (simplex-type). Ten tweede kwam naar voren, dat relaxatie inderdaad de beste therapie voor de 'alpha-'types' was en focal concentration imagery voor de beta-'types'. Elke groep verbeterde op een zeer significant niveau (p kleiner dan 0.001), vergeleken met een minimal-treatment controle groep. Er is echter een technische onvolkomenheid bij deze onderzoekingen aan te wijzen: het design was niet factorieel: De helft van de alpha groep had behandeld dienen te worden met de voor haar geschikte therapie (relaxation training) èn de andere helft met de voor haar ongeschikte therapie (focal concentration imagery). Hetzelfde geldt voor de beta groep. Door het 'Headache Research Project' werd die tekortkoming ook gezien. De onderzoekers verklaarden deze tekortkoming echter niet ontvankelijk, gezien de ongepubliceerde 'pilot'-studies die ze in verband met dit probleem verricht hadden. Naar onze mening is dit echter niet voldoende. Om de wetenschappelijke gemeenschap en in het bijzonder de sceptici te overtuigen die dit 'pilot'-werk als 'niet-definitief' zullen karakteriseren, dient men de resultaten te valideren d.m.v. het gebruik van een factorieel design. En dit is het doel van ons onderzoek. Onze interesse voor dit type onderzoek werd gewekt door ons werk m.b.t. therapeugene, nonspecifieke factoren en indicatiestelling in psychotherapie, en de resultaten van het 'Headache Research Project'. We zijn tot de conclusie gekomen, dat het niet langer verstandig is om een en dezelfde methode bij alle patiënten ongeacht persoonlijkheidskarakteristieken (die zeer belangrijke nonspecifieke variabelen zijn) toe te passen. De resultaten van het Project en het feit dat er zo vele spanningshoofdpijnlijders zijn, heeft ertoe geleid dat we spanningshoofdpijnlijders als doelgroep genomen hebben voor ons onderzoek.

Talrijke assessmentmethoden staan ons ter beschikking om de alpha van de beta types te onderscheiden. Kelly's Role Construct Repertory Test is een daarvan. Enkele schalen van de Nederlandse NPV en BV correleren hoog met de uitslag hierop. Deze twee testen werden dan ook gebruikt voor dit doel. Verder hebben Bieri & Barron de MMPI met succes voor dit doel gebruikt en speciale schalen ontworpen die de twee groepen

zeer betrouwbaar kunnen onderscheiden.
Om alpha en beta types zo "zuiver" mogelijk te houden, worden enkel patiënten voor therapie aangenomen, die op alle drie schalen consistent hetzelfde patroon vertonen.
Onze onderzoeksopzet heeft natuurlijk ethische consequenties: De helft van de patiënten in zowel de beta als de alpha groepen krijgt een voor haar irrelevante behandeling. Om onze therapieën effectiever te maken in de toekomst is dit echter onvermijdelijk (vgl. van Praag, 1977). Bovendien is het zo, dat de patiënten in de controle groepen na de behandeling op een evaluatiebijeenkomst zullen komen, waar hen de mogelijkheid tot additionele therapie geboden wordt.
Wat betreft de twee therapieën: Beide zijn verwant aan Yoga en autogene training. De relaxation therapie is eenvoudig toe te passen, en je bent er misschien zelfs al bekend mee. De focal concentration imagery therapie is even gemakkelijk te leren. Zoals reeds eerder gezegd, bestaat deze uit focal training (vergelijkbaar met de methode van Gendlin) en oefening in het je voor de geest halen van steeds complexere dingen en situaties (training in imagery building). Over beide therapie-aanpakken zul je later gedetailleerde instructies krijgen. Bovendien zal met beide therapiemethoden geoefend worden.
Ook al zul je zo veel mogelijk structuur aangeboden krijgen (zonder daarbij je spontaniteit in gevaar te brengen natuurlijk), toch zul je zelf enige ervaring en aanlooptijd in de therapie moeten krijgen om je op je gemak te voelen bij het uitvoeren ervan. Daarom bestaat de eerste helft van elke sessie uit het met de patiënt bespreken van rondom een bepaald thema centrerende clusters van NSC-testitems (bestaande uit onvolledige zinnen die door de patiënt zijn afgemaakt). Dit is niet alleen bedoeld om jou de gelegenheid te geven je in te werken en je op je gemak te voelen. Het heeft nog een andere functie: Zoals je weet, associeert de gemiddelde Nederlander therapie met het praten over persoonlijke dingen. De testitems en de antwoorden van de patiënten erop, liggen op dat vlak. Je zult zien, hoe vaak ze zich verwonderen waarom ze nu juist dàt ingevuld hebben; waarom ze met sommige items moeite hadden; dat zulke items vaak veel associaties en emoties oproepen, etc. Zulk een aanpak dient er bovendien voor de motivatie van de patiënt om naar de therapie te blijven komen op peil

te houden: De therapie dient daarom enigszins aan te sluiten bij de verwachtingen van de patiënt. Verder heeft dit gespreksdeel tot doel de patiënten wat emotioneler te maken, het arousal niveau van hen te verhogen (dit in beperkte mate natuurlijk: het tot paniek hoogte laten stijgen ervan heeft geen zin). Het is immers belangrijk dat de patiënt ook (en vooral) in dergelijke emotionele situaties (die vaak de hoofdpijn oproepen en verergeren) de nieuwe vaardigheid kan toepassen; een vaardigheid die door het dagelijks thuis oefenen op den duur een automatisme zal worden. Aangezien zowel negatieve als positieve emoties bij spanningshoofdpijnlijders een verhoogde kans op hoofdpijn geven, liggen de testitems zowel op positief als negatief vlak.

(Aanvulling bij experiment II)

<u>En verder</u>:

Bovenstaand onderzoek is in de periode dec. 1980 - juni 1981 uitgevoerd. De resultaten bevestigden de uitkomst van het onderzoek aan de Universiteit van Missouri.

De bedoeling nu is:

a) Replikatie hiervan onder <u>striktere</u> condities (een aantal therapeuten namen bij het eerste experiment nogal wat vrijheid t.a.v. het therapiedesign, met name wat betreft de therapie van de patiënt in de controle condities. Deze afwijkingen willen we dit keer kleiner houden of op zijn minst zo nauwkeurig mogelijk meten (door self-monitoring van de de therapeut na elke sessie) om hiervoor, indien nodig, een statistische correctie op toe te passen.

b) We willen dit keer niet alleen therapieoutcome bestuderen, maar ook inzicht verkrijgen in de proces-aspekten van therapie. Reden waarom we zowel patiënt als therapeut tweemaal gedurende de therapie de Barrett-Lennard Relatie-vragenlijst zullen afnemen.
   Dit is exploratief onderzoek. De bedoeling is, dat de resultaten hiervan hypothesen zullen genereren voor verdere experimenten.

Naam:
Datum:
Leeftijd:

Instructie: Op de volgende bladzijden vindt U enige onvolledige zinnen. Wilt U elke zin lezen en met de eerste gedachte, die in U opkomt de zin afmaken? Maak er lopende zinnen van, maar besteed niet teveel tijd aan een zin. Werk snel en probeer alle zinnen af te maken. Draait U nu de bladzijde maar om.

1. *Als ik iets doe waarover ik me schaam, dan zou ik* .....

2. *Iemand die verliefd wordt* .....

3. *Omwille van moeder, zou ik* .....

4. *Zo'n 5 jaar geleden was ik anders dan ik nu ben, nl.* .....

5. *Mijn meest kwetsbare lichaamsdeel is* .....

6. *Mijn beste eigenschap is* .....

7. *Trouwen* .....

8. *Ik voel me in de put als* .....

9. *De meeste mannen denken dat vrouwen* .....

10. *Telkens als ik bij mijn moeder ben, dan voel ik* .....

11. *Als ik in de spiegel kijk,* .....

12. *Ik zou iemand kunnen haten die* .....

13. *De plezierigste dromen zijn* .....

14. *Er zouden minder echtscheidingen zijn, als* .....

15. *Wat mijn geweten het meest dwars zat, was* .....

16. *Mijn eerste reactie op haar was* .....

17. *Voor vader, zou ik* .....

18. *Het liefste wil ik* .....

19. Er is niets wat mij zo woedend heeft gemaakt als .....

20. De dingen waar ik goed in ben .....

21. Geloof in God .....

22. Als ik teleurgesteld ben .....

23. Sexuele gemeenschap .....

24. Ouders zouden moeten weten .....

25. Mijn grootste wens is .....

26. Soms maak ik mij zorgen over .....

27. Het leek onmogelijk, dat ik .....

28. Mijn levensfilosofie is .....

29. Ik voel me schuldig over .....

30. Soms vind ik dat sex .....

31. Toen mijn vader thuis kwam .....

32. Mijn diepste verlangen .....

33. Ik heb me nog nooit zo geërgerd, als .....

34. Ik voel mij het gelukkigst, wanneer .....

35. Wat ik eigenlijk van studie vind .....

36. Als ik eraan terug denk, schaam ik me dat .....

37. Een man haat een vrouw die .....

38. *Ouders zouden zich minder zorgen maken, als* .....

39. *Men vindt mij* .....

40. *Je beste vriend kan je irriteren als* .....

41. *Ik ben het meest trots op* .....

42. *Er is zoveel misdadigheid, omdat* .....

43. *De ongelukkigste ervaring uit mijn leven was* .....

44. *Een vrouw wil een man die* .....

45. *Kinderen kunnen er nooit zeker van zijn dat hun ouders* .....

46. *Telkens als ik aan de toekomst denk* .....

47. *Mijn grootste angst is* .....

48. *Het belangrijkste in het leven is* .....

49. *Als iemand aan een ander een raad vraagt en hij volgt deze raad niet op, dan kan die ander* .....

50. *Ik maak mij het meeste zorgen over* .....

51. *Haar reactie, nadat hij met haar naar bed was gegaan, was* .....

52. *Toen mijn moeder thuis kwam* .....

53. *Men denkt meestal dat ik* .....

54. *Ik voelde mij niet op mijn gemak, want* .....

55. *Het soort mensen dat mij het beste ligt* .....

56. *Het is verkeerd om* .....

CATEGORY 1: DESPAIR, HOPELESSNESS, GUILT & SHAME.

item

1. *Als ik iets doe waarover ik me schaam, dan zou ik* 1

2. *Als ik eraan terug denk, schaam ik me dat* 36

3. *Ik voel me in de put als* 8

4. *De ongelukkigste ervaring uit mijn leven was* 43

5. *Wat mijn geweten het meest dwars zat, was* 15

6. *Als ik teleurgesteld ben* 22

7. *Ik voel me schuldig over* 29

8. *Ik maak mij het meeste zorgen over* 50

CATEGORY 2: SEX AND SEX ROLES.

	item
1. *Iemand die verliefd wordt*	2
2. *Haar reactie, nadat hij met haar naar bed was gegaan, was*	51
3. *Soms vind ik dat sex*	30
4. *Sexuele gemeenschap*	23
5. *De meeste mannen denken dat vrouwen*	9
6. *Mijn eerste reactie op haar was*	16
7. *Een man haat een vrouw die*	37
8. *Een vrouw wil een man die*	44

CATEGORY 3: ATTITUDE TOWARD PARENTS.

item

1. *Toen mijn vader thuis kwam* 31

2. *Toen mijn moeder thuis kwam* 52

3. *Ouders zouden moeten weten* 24

4. *Kinderen kunnen er nooit zeker van zijn dat hun ouders* 45

5. *Voor vader, zou ik* 17

6. *Omwille van moeder, zou ik* 3

7. *Ouders zouden zich minder zorgen maken , als* 38

8. *Telkens als ik bij mijn moeder ben, dan voel ik* 10

CATEGORY 4: ASPIRATIONS, FUTURE ORIENTATION, SELF CONCEPT.

item

1. *Mijn grootste wens is* 25

2. *Het liefste wil ik* 18

3. *Zo'n 5 jaar geleden was ik anders dan ik nu ben, nl.* 4

4. *Mijn diepste verlangen* 32

5. *Telkens als ik aan de toekomst denk* 46

6. *Als ik in de spiegel kijk,* 11

7. *Men vindt mij* 39

8. *Men denkt meestal dat ik* 53

CATEGORY 5: FEARS, CONCERNS, PERSONAL GAPS.

item

1. *Mijn grootste angst is* 47

2. *Ik heb me nog nooit zo geërgerd, als* 33

3. *Je beste vriend kan je irriteren als* 40

4. *Ik zou iemand kunnen haten die* 12

5. *Er is niets wat mij zo woedend heeft gemaakt als* 19

6. *Soms maak ik mij zorgen over* 26

7. *Ik voelde mij niet op mijn gemak, want* 54

8. *Mijn meest kwetsbare lichaamsdeel is* 5

CATEGORY 6: POSITIVE REACTIVITY, POTENTIALS FOR MASTERY OF PROBLEMS.

	item
1. Ik ben het meest trots op	41
2. Mijn beste eigenschap is	6
3. De plezierigste dromen zijn	13
4. Het soort mensen dat mij het beste ligt	55
5. De dingen waar ik goed in ben	20
6. Ik voel mij het gelukkigst, wanneer	34
7. Het belangrijkste in het leven is	48
8. Het leek onmogelijk, dat ik	27

CATEGORY 7: INTERPERSONAL PERCEPTIONS & VALUES.

item

1. *Er zouden minder echtscheidingen zijn, als* 14

2. *Mijn levensfilosofie is* 28

3. *Geloof in God* 21

4. *Wat ik eigenlijk van studie vind* 35

5. *Er is zoveel misdadigheid, omdat* 42

6. *Trouwen* 7

7. *Als iemand aan een ander een raad vraagt en hij volgt deze raad niet op, dan kan die ander* 49

8. *Het is verkeerd om* 56

Appendix D

BIOGRAFISCHE VRAGENLIJST

Doel

Het doel van deze vragenlijst is het verkrijgen van duidelijke informatie over u, uw hoofdpijn en uw achtergrond. Door deze vragen zo volledig en zo nauwkeurig mogelijk te beantwoorden, zult u er aan meewerken het behandelingsprogramma te vergemakkelijken en te versnellen. Uw antwoorden zullen als strikt vertrouwelijk worden beschouwd.

1. Algemeen

Datum:

Naam:

Adres:

Telefoon:

Geboortedatum:

Beroep:

Burgerlijke staat (kruis het juiste antwoord aan):

0 ongetrouwd	0 gescheiden van tafel en bed
0 verloofd	0 gescheiden
0 getrouwd	0 aan het scheiden
0 samenwonend	0 weduwe

Naam huisarts:
adres:
telefoon:

2. De klacht (hoofdpijn)

a. Hoe lang hebt u er al last van? (s.v.p. aantal jaren óf maanden noemen):

---

b. Hoeveel dagen per maand hebt u gemiddeld ongeveer hoofdpijn?

---------- dagen

c. Voelt u de hoofdpijn van te voren aankomen?

Zo ja, waaraan merkt u dat de hoofdpijn eraan komt?

---

d. Waar is de hoofdpijn gelocaliseerd? Is hij bijvoorbeeld maar aan één kant van het hoofd? In de nek? In de buurt van de slapen? etc.

---

e. Hoe zou u de hoofdpijn beschrijven? (aankruisen wat van toepassing is):

O als een knellende band om het hoofd
O stekend
O kloppend
O drukkend
O borend
O snijdend
O anders, namelijk: ----------

f. Voelt u zich misselijk vlak vóór of tijdens de hoofdpijn?

Zo ja, vaak of soms? ----------

g. Moet u braken vlak vóór of tijdens de hoofdpijn?

Zo ja, vaak of soms? ----------

h. Bent u snel geïrriteerd als u hoofdpijn heeft?

---

i. Hangt uw hoofdpijn samen met de menstruatie?

Zo ja, hoe? ----------

j. Zijn er familieleden, die veel last van hoofdpijn hebben?

Zo ja, welke (geen aangetrouwde noemen):

---

k. Bent u al eerder in behandeling geweest voor uw hoofdpijn?

Zo ja, waar? bij wie? wanneer? wat waren de resultaten?

1. _____
2. _____
3. _____
4. _____
5. _____

l. Zijn er dingen, waarvan u weet dat ze bij u hoofdpijn veroorzaken of die de hoofdpijn erger doen worden?

Zo ja, graag toelichten: _____

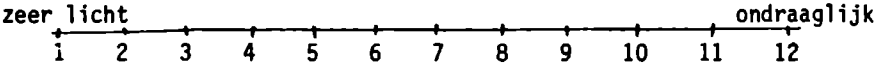
---

m. Bent u nu nog in behandeling voor uw hoofdpijn? _____

Zo ja,waar, bij wie, sinds wanneer, en wat zijn de resultaten tot nu toe?

---

n. Hoe hevig is de hoofdpijn gewoonlijk? (Geef een schatting op de hieronder gegeven schaal; omcirkel één van de getallen: hoe hoger het getal, hoe heviger de hoofdpijn):



o. Welk resultaat met betrekking tot uw hoofdpijn verwacht u van de therapie(kruis aan welk antwoord het beste uw verwachting weergeeft; s.v.p. slechts één antwoord aankruisen):

O tegen het einde van de therapie is de hoofdpijn helemaal weg

O tegen het einde van de therapie is de hoofdpijn bijna helemaal we

O tegen het einde van de therapie is de hoofdpijn véél minder dan vóór de behandeling

O tegen het einde van de therapie is de hoofdpijn ongeveer de helft minder

O tegen het einde van de therapie is de hoofdpijn een beetje minder

O tegen het einde van de therapie is de hoofdpijn hetzelfde gebleve

O tegen het einde van de therapie is de hoofdpijn erger

p. Hoe voelt u zich nu u besloten hebt voor uw hoofdpijn in therapie te gaan (s.v.p. twee antwoorden aankruisen):

O blij
O opgelucht
O verdrietig
O onzeker
O vol verwachting
O twijfelend

3. Persoonlijke gegevens

a. Toestand van uw moeder gedurende zwangerschap? (voor zover u bekend):

---

b. Kruis aan wat van toepassing was op uw jeugd:

O nachtmerries
O duimzuigen
O angst
O bedwateren
O nagelbijten
O gelukkige jeugd
O slaapwandelen
O stotteren
O ongelukkige jeugd

c. Gezondheid gedurende de jeugd (tot 18 jaar)?
Lijst van ziekten:

---

d. Gezondheid gedurende de leeftijd vanaf 18e jaar?
Lijst van ziekten:

---

e. Wat is uw lengte: _____

Wat is uw gewicht: _____

f. Zijn er chirurgische ingrepen geweest? _____

Zo ja, welke en op welke leeftijd? _____

---

g. Hoe vaak gaat u gemiddeld naar de huisarts?

gemiddeld:_____keer per maand

of

-------keer per jaar

h. Hoe vaak gaat u gemiddeld naar een specialist?

gemiddeld:_____keer per maand

of

_____keer per jaar

i. Als u ziek bent, hoe snel wordt u dan meestal beter? (Maar één antwoord aankruisen s.v.p.):

O veel sneller dan anderen

O sneller dan anderen

O even snel als anderen

O minder snel dan anderen

O veel minder snel dan anderen

j. Is het wel eens voorgekomen dat u door een behandeling (van een arts of therapeut) nog zieker bent geworden? _____

Zo ja, wanneer? en wat waren toen de klachten?:

---

k. Als u gespannen bent of problemen hebt en er zelf niet meer uitkomt, welke behandeling zou u kiezen(één antwoord s.v.p.):

O medicijnen (van huisarts of specialist)
O therapeutische gesprekken met een psycholoog
O uitpraten met vriend of vriendin
O hulp zoeken bij pastoor of dominee

l. Hebt u wel eens een ongeluk of ongeval gehad? _____

Zo ja, welke en wanneer: _____

_____

_____

m. Waarvoor bent u het meeste bang? Geef een lijst van de voor u 5 grootste angsten:

1. _____

2. _____

3. _____

4. _____

5. _____

n. Kruis steeds aan wat op u van toepassing is:

O hartkloppingen
O darmstoornissen
O nachtmerries
O voel me gespannen
O ben niet in staat me te ontspannen
O houd niet van weekends en vakantie
O kan geen vrienden maken
O ben snel afgeleid
O verander steeds van baan
O ben godsdienstig
O financiële problemen
O ben makkelijk beïnvloedbaar
O gebruik drugs
O kan mijn gedachten niet stilzetten
O duizelig
O maagklachten
O ben vermoeid
O slik kalmerende middelen
O voel me paniekerig
O zelfmoord-gedachten
O seksuele problemen
O snel geïrriteerd
O ben teruggetrokken
O kan niet tegen kritiek
O ben flexibel
O ben te eerzuchtig
O ben te vertrouwen
O heb minderwaardigheidsproblemen
O heb geheugenproblemen
O ben een optimist
O vergis me vaak
O flauw vallen
O heb nooit honger
O kan niet slapen
O alcoholmisbruik
O trillende handen
O neem verdovende middelen
O kan me niet concentreren
O neb teveel problemen
O kan geen beslissingen nemen
O toestand thuis is slecht
O ben een pessimist
O kan niet van vrije tijd genieten
O kan moeilijk lang aandacht richten op iets
O werk veel
O verlegen met mensen
O ben een perfectionist

o. Kruis van onderstaande woorden aan, wat op u van toepassing is:

O waardeloos	O lelijk
O nutteloos	O misvormd
O een "nul"	O niet aantrekkelijk
O leven is leeg	O afstotend
O onaangepast	O terneergeslagen
O stom	O alleen
O niet bekwaam	O niet geliefd
O onnozel	O verkeerd begrepen
O kan niets goed doen	O lastig
O schuldig	O rusteloos
O slecht	O verward
O verschrikkelijke gedachten	O in conflict
O vijandig	O vol spijt
O vol haat	O waardevol
O angstig	O geliefd
O opgewonden	O intelligent
O laf	O aantrekkelijk
O paniekerig	O te vertrouwen
O agressief	O bedachtzaam

p. Huidige interesses, liefhebberijen en aktiviteiten:

q. Hebt u nog een opleiding of cursus gevolgd nà de lagere school?

Zo ja, welke, en wanneer:

1. ---
2. ---
3. ---
4. ---
5. ---

r. Maakt u gemakkelijk vrienden: _____ (Vul in: ja of nee)

Houdt u ze dan ook langere tijd:_____ (")

4. Gegevens met betrekking tot werk

a. Wat voor soort werk doet u nu:

---

b. Wat voor soort banen hebt u in het verleden gehad?

Aard van het werk:	vanaf:	tot aan:
1.		
2.		
3.		
4.		
5.		

c. Bent u tevreden met het werk wat u nu doet? (Zo niet, waarom bent u ontevreden?)

---

5. Informatie met betrekking tot seksualiteit

a. Wanneer en hoe deed u uw eerste kennis op over de seksualiteit?

---

b. Wanneer werd u zich voor het eerst bewust van uw seksuele gevoelens?

---

c. Hebt u ooit angst of schuldgevoelens gehad, die te maken hadden met de seksualiteit, bv. met zelfbevrediging?(Geef dit op onderstaande schaal aan:)

geen angst en schuldgevoelens — veel angst en schuldgevoelens

1 2 3 4 5 6 7 8 9 10 11 12

d. Zijn bepaalde bijzonderheden u bijgebleven van uw eerste of latere seksuele ervaring(en)?:

---

e. Is uw huidige seksuele leven bevredigend?

onbevredigend — zeer bevredigend

1 2 3 4 5 6 7 8 9 10 11 12

6. Informatie over uw tegenwoordige huwelijk (alleen invullen als u gehuwd bent of samenwoont.)

a. Hoe lang bent u getrouwd of woont u samen?

---

b. Hoe lang kende u uw partner voordat u trouwde of ging samenwonen?

---

c. Leeftijd van echtgenoot/partner: _____

d. Beroep van echtgenoot/partner: _____

e. Omschrijf in het kort uw echtgenoot/partner als persoonlijkheid; zijn voor u belangrijke eigenschappen en gedragingen:

---

f. Hoe staat u t.o.v. uw schoonfamilie?

---

g. Hoeveel kinderen hebt u uit uw tegenwoordige huwelijk? (leeftijd, jongen of meisje):

---

h. Heeft een of meer van deze kinderen speciale problemen?

6.1. Informatie over uw huwelijk in het geval dat uw echtgenoot is overleden of dat u gescheiden bent:

a. Hoe lang bent u getrouwd geweest?

Van..... tot .....

b. Hoe lang kende u uw partner voordat u trouwde?

---

c. Geboortedatum van ec htgenoot: _____

d. Beroep van echtgenoot: _____

e. Omschrijf in het kort uw echtgenoot als persoonlijkheid; zijn voor u belangrijke eigenschappen en gedragingen:

---

f. Hoeveel kinderen hebt u uit dit huwelijk? (leeftijd, jongen of meisje)

---

g. Heeft een of meer van deze kinderen speciale problemen?

---

7. Familiegegevens

a. Vader:

In leven of overleden?

Indien overleden, wat was uw leeftijd toen hij overleed?

waaraan is uw vader gestorven?

Indien uw vader nog leeft, wat is zijn leeftijd?

Beroep van vader?

Gezondheid van vader?

b. Moeder:

In leven of overleden?

Indien overleden, wat was uw leeftijd toen zij overleed?

Waaraan is uw moeder gestorven?

Beroep van moeder?

Gezondheid van moeder:

c. Broers en zusters:

Aantal broers: Leeftijd(en):

Aantal zusters: Leeftijd(en):

Appendix E

NAAM:

DATUM:

HOOFDPIJN: ja / nee (omcirkel wat van toepassing is).

Zo ja: Van wanneer tot wanneer duurde de hoofdpijn? (Omcirkel de tijd waarop de hoofdpijn ongeveer begon en wanneer hij ongeveer ophield).

0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 uur

Hoe hevig, intens was de hoofdpijn vandaag gemiddeld? (Omcirkel het getal, dat het beste de hevigheid weergeeft. Hoe lager het getal hoe minder hevig de hoofdpijn, hoe hoger het getal hoe heviger de hoofdpijn: bijv. 1= zeer lichte hoofdpijn, 12= ondraaglijke hoofdpijn).

1 2 3 4 5 6 7 8 9 10 11 12

Gebruikte U vandaag hoofdpijntabletten, poeders en dergelijke?.....
Zo ja: Welke en hoeveel? .....
.....

Appendix F EVALUATIEFORMULIER: Patiënt

NAAM:

GEBOORTEDATUM:

Zoals U weet kan elke behandeling nog verbeterd worden. Het doel van deze vragenlijst is om van Uw antwoorden te leren, op welke manieren we de behandeling voor de patiënten die nog gaan komen, kunnen verbeteren.

1.a. Had U zelf de advertentie gezien, waarin stond dat op het Psychologisch Laboratorium hoofdpijn-therapie gegeven wordt?

O ja
O nee

b. Zo nee, wie heeft U er dan op attent gemaakt?

O een familielid
O een kennis
O iemand anders, namelijk .....

2. Wat zouden we, behalve door advertenties in De Gelderlander en De Brug, nog meer kunnen doen om meer mensen op de mogelijkheid te wijzen, dat ze op het Psychologisch Laboratorium hoofdpijn-therapie kunnen krijgen?

.....

.....

3. Als een kennis van U ook spanningshoofdpijn heeft of krijgt en aan U vraagt wat ze er het beste tegen doen kan: Zoudt U deze kennis dan de behandeling die Uzelf bij ons gekregen heeft, aanraden?

O natuurlijk
O waarschijnlijk wel
O misschien wel
O dat weet ik nog niet
O misschien niet
O waarschijnlijk niet
O nee

4. Kwamen de tijden waarop de behandeling plaatsvond U goed uit?

O ja, altijd
O meestal wel
O soms wel, soms niet
O meestal niet
O nee

5. Hoe is het de laatste tijd met de hoofdpijn?

O helemaal weg
O bijna helemaal weg
O veel minder dan vóór de behandeling
O ongeveer de helft minder
O een beetje minder
O hetzelfde
O erger

6. Hoe vond U de duur van de therapie?

O te lang
O goed
O te kort

7. Hoe gingen de oefeningen thuis?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

8. Hoe vaak deed U de oefeningen thuis?

O dagelijks of meer
O 6 dagen per week
O 5 dagen per week
O 4 dagen per week
O minder dan 4 dagen per week

9. Kon U goed met de therapeut(e) opschieten?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

10. Welke dingen bevielen U het beste aan de behandeling?

.....

.....

11. Welke dingen bevielen U het minste aan de behandeling?

.....

.....

12. Elke behandeling kan verbeterd worden. Wat hadden we nog meer (of anders) kunnen doen om de behandeling voor U (en dus ook voor mensen die op U lijken) effectiever te maken?

.....

.....

Naam: .....

Datum: .....

1. Hoe heb je je werkzaamheden hier ervaren?

O zeer positief
O positief
O neutraal
O negatief
O zeer negatief

2. Wat vond je het meest positieve aan je werkzaamheden als therapeut?

.....

.....

.....

.....

3. Wat vond je het minst positieve aan je werkzaamheden als therapeut?

.....

.....

.....

.....

4.a. Heb je iets gemist in de inwerkingsperiode?

O ja
O nee

b. Zo ja, wat?

.....

.....

.....

.....

5.a. Had je zelf al enige ervaring met één of beide technieken, of met eraan verwante Yoga technieken?

O ja
O nee

b. Zo ja, welke? .....

c. Zo ja, hoeveel ervaring? .....

6. Geef een punt (van 1 tot 10) voor de volgende werkzaamheden, die je als studentassistent verricht hebt (hoe hoger het punt, des te positiever je ervaring van het betreffende onderdeel van je werkzaamheden toen je deze verrichtte).

-- de eerste therapie-dag ..... _____

-- het therapie doen zelf:

- in het algemeen ..... _____
- het eerste, gespreksgedeelte van elk uur (de NSC-bespreking, e.d.) ..... _____
- het geven van ontspanningsoefeningen
  - -- in het algemeen ..... _____
  - -- in de OO conditie ..... _____
  - -- in de OC conditie ..... _____
- het geven van concentratieoefeningen
  - -- in het algemeen ..... _____
  - -- in de CC conditie ..... _____
  - -- In de CO conditie ..... _____

-- het 's ochtends therapie geven: ..... _____

-- het 's middags therapie geven: ..... _____

-- het 's avonds therapie geven: ..... _____

-- het meer dan 4 therapieën op één dag geven: ..... _____

-- therapie geven met veel vrije uren ertussen op één dag: ..... _____

7. Wat vind je beter voor de volgende "generatie" therapeuten (omcirkel het antwoord dat jou het meest aanspreekt):

A. Ze krijgen een 'vuurdoop' van 3 à 4 therapieën achter elkaar op de eerste dag
B. Ze krijgen de eerste dagen maar telkens 1 of hoogstens 2 patiënten per dag, waarna de hoeveelheid opgevoerd wordt.

8.a. Had je méér patiënten in de CO en/of OC condities dan in de CC en/of OO condities?

O ja
O nee

b. Zo ja, vond je dat vervelend?

O ja
O nee

c. Moeten we door 'matching' of iets dergelijks, zorgen dat dit in een volgende fase van het onderzoek vermeden wordt, ten behoeve van de volgende generatie therapeuten, of denk je dat dat niet nodig is?

O wel nodig
O niet nodig

9. Wat vind je van de kwaliteit van de opzet en organisatie van het onderzoek?

.....

.....

.....

.....

.....

10. Heb je zelf enige suggesties m.b.t. de opzet van het onderzoek (houd hierbij rekening met je ervaringen met de patiënten):

.....

.....

.....

.....

.....

11. Hoe sta je t.o.v. de ethische aspekten van het werken met experimentele controle en wachtlijst groepen, zoals wij die gebruikt hebben? (Houd hierbij in je antwoord rekening met het feit dat de patiënten in de controle condities na de evaluatieperiode de mogelijkheid hebben alsnog de juiste therapie te krijgen):

.....

.....

.....

.....

.....

12. Heb je enkele suggesties m.b.t. de ethische aspekten van het onderzoek, die de wetenschappelijke kwaliteit van het onderzoek niet in gevaar brengen?:

.....

.....

.....

.....

.....

13. Welke patiënten zijn naar jouw mening het meest vooruitgegaan?
Namen:

14. Welke patiënten zijn naar jouw mening het minst vooruitgegaan?
Namen:

15. Heb je nog suggesties of opmerkingen die in het bovenstaande niet aan de orde zijn gekomen, dan hier graag vermelden:

.....

.....

.....

.....

.....

Instructie

ONTSPANNINGSOEFENINGEN

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NIJMEGEN

Ontspanningsoefeningen

Wenken:

1. Ga in een gemakkelijke stoel zitten of op bed liggen, de benen naast elkaar (dus niet gekruist).
2. Zorg dat het hele lichaam en vooral de armen goed ondersteund zijn.
3. Let bij de oefeningen vooral op het verschil in gevoel tussen het spannen van de spieren en het loslaten van de spanning daarna.
4. Doe de oefeningen daarom niet te vlug achter elkaar.
5. Bij elke oefening spannen we de spieren ongeveer 3 seconden.
6. Laat het ontspannen zijn van de spieren steeds langer duren dan het aanspannen.
7. Zorg ervoor dat na elke oefening de ademhaling weer langzaam, rustig en gelijkmatig wordt.
8. Doe de oefeningen met gesloten ogen: Dit verhindert dat U al te snel afgeleid wordt en bevordert het tot stand komen van de ontspanning.
9. Goed ontspannen is iets wat U geleidelijkaan leert.
   Het is daarom belangrijk dat er elke dag geoefend wordt.

I. Oefeningen voor handen en armen.

1. Bal de vuist van Uw schrijfhand.....en los.
   Dit doen we nog een keer.....en los.
2. Bal de vuist van de andere hand.....en los.
   Nog een keer.....en los.
3. Bal allebei de vuisten.....en los.
4. Buig de ellebogen en span de armspieren...en los.
5. Strek de armen, spreidt Uw vingers zo ver mogelijk.....en los.
   Dit doen we nog een keer.....en los.

II. Oefeningen voor voeten, benen en billen.

1. Druk Uw tenen zo ver mogelijk van U af (naar voren).....en los.
   Nog een keer.....en los.
2. Trek Uw tenen naar U toe (houdt de benen hierbij gestrekt).....en los.
   Nog een keer.....en los.
3. Houdt de benen gestrekt en span de bilspieren en de bovenbeenspieren (de heupen komen hierdoor iets omhoog).....en los.
   Nog een keer.....en los.

III. Oefeningen voor buik-, rug-, borst-, en schouderspieren.

Opm.: a. Omdat de hier genoemde spiergroepen nogal snel vermoeid raken, doen we deze oefeningen elk maar één keer.
b. Let er vooral op dat de ademhaling na elk van de oefeningen weer rustig, langzaam en gelijkmatig wordt.

1. Span Uw buikspieren door de buik uit te zetten (d.w.z. een "ballonbuik" te maken)...en los.
2. Span Uw buikspieren door Uw buik in te trekken .....en los.
3. Span de borstspieren door eerst heel diep in te ademen, waardoor de borstkast uitzet en dan (terwijl de borstkast uitgezet blijft) gaat U heel oppervlakkig in en uit ademen.....en los.
4. Span de schouder- en schouderbladspieren door de schouders naar voren te duwen, waardoor de borstkast hol en de rug bol wordt.....en los.

IV. Oefeningen voor nek-, en hoofdspieren.

Opm.: Vooral met het ontspannen van de voorhoofds-, oog-, en kaakspieren gaat ook een psychische ontspanning gepaard.

1. Span de nekspieren door de kin op de borst te drukken. Houdt de nekspieren gespannen. Draai dan met het hoofd langzaam naar rechts, dan naar links, tenslotte weer terug naar het midden (terug in de uitgangspositie).....en los.
2. Span de kaakspieren door de kiezen op elkaar te drukken.....en los.
3. Span de tong door deze tegen de achterkant van de boventanden te duwen.....en los.
4. Span de lippen door deze vóór de tanden op elkaar te persen.....en los.
5. Span de oogspieren en wenkbrauwen door de ogen stijf dicht te knijpen.....en los.
6. Span Uw voorhoofdsspieren door boos te kijken (d.w.z. door de wenkbrauwen omlaag te doen) .....en los.
7. Span Uw voorhoofdsspieren door verbaasd te kijken (d.w.z. door de wenkbrauwen omhoog te doen) ....en los.

V. Slot.

Tot slot spannen we tegelijkertijd een groot aantal spiergroepen aan. Dit doen we als volgt: We brengen de gestrekte armen vóór het lichaam en drukken de handpalmen tegen elkaar. Tegelijkertijd houden we de benen gestrekt en drukken de zijkant van de voeten tegen elkaar, en drukken we de kiezen op elkaar waardoor de kaakspieren gespannen worden.
Dit doen we nu.....en los.

Na deze oefening blijven we ongeveer 5 minuten lekker ontspannen liggen, met de ogen gesloten.

Opm.: Sta nà de oefeningen niet te snel op, niet te abrupt, maar open eerst de ogen, ga dan rechtop zitten en sta dan pas op.
Wanneer men te abrupt opstaat, bestaat er grote kans dat men duizelig en draaierig in het hoofd wordt.

Instructie

CONCENTRATIEOEFENINGEN

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NIJMEGEN

CONCENTRATIEOEFENINGEN

Wenken:

1. Zorg ervoor, dat U tijdens de oefeningen niet gestoord wordt. Zet bijvoorbeeld radio of TV uit, of doe de oefeningen in een kamer waar U het geluid ervan niet hoort.
2. De tijdsduur, die achter elke oefening staat, is bij benadering. Op een minuut meer of minder komt het niet aan.
3. Vooral in het begin zullen Uw gedachten bij een oefening afdwalen. Zodra U dit merkt, moet U zo snel mogelijk de concentratieoefening hervatten.
4. Goed leren concentreren is iets wat U geleidelijk leert. Het is daarom van belang dat U dagelijks oefent.

SERIE I.

Oefening 1 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. Stel U voor, dat U in Uw eigen huiskamer bent.
3. Kijk in gedachten in de kamer rond en neem bewust waar wat U 'ziet'.
4. Probeer zo levendig mogelijk te zien, wat U zou zien, wanneer U werkelijk in de kamer was.
5. Let ook op de details, zoals asbakken, planten, tafelkleedjes.
6. Doe de ogen weer open.

Oefening 2 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich weer voor dat U in Uw eigen huiskamer bent.
3. U staat in gedachten op en wandelt richting buitendeur.
4. U neemt tijdens deze wandeling zo veel mogelijk waar wat U op Uw weg tegenkomt. U doet over de wandeling even lang als hij in werkelijkheid zou duren.
5. Wanneer U bij de buitendeur aangekomen bent, gaat U naar buiten en loopt tot aan de straat.
6. Vervolgens draait U zich om en wandelt dezelfde weg weer terug in gedachten, tot U weer terug bent in de stoel van waaruit U de wandeling begonnen bent.
7. U opent de ogen weer.

SERIE II.

Oefening 1. (duur: 6 minuten)

1. Neem een voorwerp. Plaats het voor U op tafel en bekijk het gedurende ongeveer 1 minuut. Concentreer U op het voorwerp en probeer aan niets anders te denken.
2. Sluit Uw ogen en tracht het voorwerp U voor de geest te halen. (1 minuut).
3. Open de ogen en bekijk het voorwerp weer. Houdt Uw gehele aandacht bij het voorwerp. (1 minuut).
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open de ogen weer.

Oefening 2. (duur: 5 minuten).

1. U legt Uw horloge voor U op tafel neer.
2. Volg de secondewijzer aandachtig in zijn bewegingen. Laat U niet afleiden en probeer Uw aandacht bij de bewegingen van de secondewijzer te houden, gedurende 1 minuut.
3. Houdt een minuut pauze.
4. Idem als onderdeel 2.
5. Idem als onderdeel 3.
6. Idem als onderdeel 2.

SERIE III.

Oefening 1.(duur: 7 minuten).

1. Plaats een vijftal kleine voorwerpen voor U op tafel. Bekijk ze gedurende 1 minuut.
2. Sluit de ogen en probeer ze U voor de geest te halen, alle vijf en zo gedetailleerd mogelijk. (1 minuut)
3. Open de ogen en controleer of U ze alle vijf onthouden heeft, of de details correct waren en probeer verdere details te onthouden. (1 minuut).
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open Uw ogen en vergelijk de voorstelling met de werkelijkheid.

Instructie

CONCENTRATIEOEFENINGEN

II

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Psychologisch Laboratorium
Montessorilaan 3
Katholieke Universiteit
NIJMEGEN

CONCENTRATIEOEFENINGEN

Wenken:

1. Zorg ervoor, dat U tijdens de oefeningen niet gestoord wordt. Zet bijvoorbeeld radio of TV uit, of doe de oefeningen in een kamer waar U het geluid ervan niet hoort.
2. De tijdsduur, die achter elke oefening staat, is bij benadering. Op een minuut meer of minder komt het niet aan.
3. Vooral in het begin zullen Uw gedachten bij een oefening afdwalen. Zodra U dit merkt, moet U zo snel mogelijk de concentratie-oefening hervatten.
4. Goed leren concentreren is iets wat U geleidelijk leert. Het is daarom van belang dat U dagelijks oefent.

==========

SERIE I. *(oefening 1 vervalt)*

Oefening 1 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. Stel U voor, dat U in Uw eigen huiskamer bent.
3. Kijk in gedachten in de kamer rond en neem bewust waar wat U 'ziet'.
4. Probeer zo levendig mogelijk te zien, wat U zou zien, wanneer U werkelijk in de kamer was.
5. Let ook op de details, zoals asbakken, planten, tafelkleedjes.
6. Doe de ogen weer open.

Oefening 2 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich weer voor dat U in Uw eigen huiskamer bent.
3. U staat in gedachten op en wandelt richting buitendeur.
4. U neemt tijdens deze wandeling zo veel mogelijk waar wat U op Uw weg tegenkomt. U doet over de wandeling even lang als hij in werkelijkheid zou duren.
5. Wanneer U bij de buitendeur aangekomen bent, gaat U naar buiten en loopt tot aan de straat.
6. Vervolgens draait U zich om en wandelt dezelfde weg weer terug in gedachten, tot U weer terug bent in de stoel van waaruit U de wandeling begonnen bent.
7. U opent de ogen weer.

Oefening 3 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich voor dat U in een straat staat waar U vaak doorloopt, bijvoorbeeld een straat die op de weg naar Uw werk ligt of op de weg naar de supermarkt waar u vaak inkopen doet.
3. Begin in gedachten te wandelen door deze straat.
4. Probeer alle dingen die in de straat te zien zijn zo duidelijk mogelijk in gedachten waar te nemen.
5. Wandel langzaam in gedachten, zodat U alles goed in U kunt opnemen.
6. U opent de ogen weer.

SERIE II *(oefening 1 vervalt)*

Oefening 1 (duur: 6 minuten).

1. Neem een voorwerp. Plaats het voor U op tafel en bekijk het gedurende ongeveer 1 minuut. concentreer U op het voorwerp en probeer aan niets anders meer te denken.
2. Sluit Uw ogen en tracht het voorwerp U voor de geest te halen. (1 minuut).
3. Open de ogen en bekijk het voorwerp weer. Houdt Uw hele aandacht bij het voorwerp. (1 minuut)
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open de ogen weer.

Oefening 2 (duur: 6 minuten).

1. Deze oefening is identiek aan oefening 1. Maar nu probeert U allerhande details aan het voorwerp waar te nemen in de eerste minuut.
2. U sluit de ogen en tracht het voorwerp inclusief de details zo goed mogelijk in Uw gedachten waar te nemen (1 minuut).
3. Open de ogen weer en controleer of U in de voorstelling de details waar U op gelet heeft ook gezien hebt. Tegelijkertijd zoekt U naar Nieuwe details om te onthouden (1 minuut).
4. Idem als onderdeel 2.
5. Idem als onderdeel 3.
6. Idem als onderdeel 2.
7. Open de ogen weer.

Oefening 3 (duur: 5 minuten)

1. U legt Uw horloge voor U op tafel neer.
2. Volg de secondewijzer aandachtig in zijn bewegingen. Laat U niet afleiden en probeer Uw gehele aandacht bij de bewegingen van de secondewijzer te houden, gedurende 1 minuut.
3. Houd een minuut pauze.
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.

SERIE III. *(oefening 1 vervalt)*

Oefening 1. (duur: 7 minuten).

1. Plaats een vijftal kleine voorwerpen voor U op tafel. Bekijk ze gedurende 1 minuut.
2. Sluit de ogen en probeer ze U voor de geest te halen, alle vijf en zo gedetailleerd mogelijk (1 minuut).
3. Open de ogen en controleer of U ze alle vijf on onthouden heeft, of de details correct waren en probeer verdere details te onthouden. (1 minuut)
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open Uw ogen en vergelijk Uw voorstelling met de werkelijkheid.

Oefening 2. (duur: 7 minuten).

Deze oefening is identiek aan oefening 1. Alléen doet U de oefening nu met 10 voorwerpen.

Instructie

CONCENTRATIEOEFENINGEN

III

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NIJMEGEN

CONCENTRATIEOEFENINGEN

Wenken:

1. Zorg ervoor, dat U tijdens de oefeningen niet gestoord wordt. Zet bijvoorbeeld radio of TV uit, of doe de oefeningen in een kamer waar U het geluid ervan niet hoort.

2. De tijdsduur, die achter elke oefening staat, is bij benadering. Op een minuut meer of minder komt het niet aan.

3. Vooral in het begin zullen Uw gedachten bij een oefening afdwalen. Zodra U dit merkt, moet U zo snel mogelijk de concentratieoefening hervatten.

4. Goed leren concentreren is iets wat U geleidelijk leert. Het is daarom van belang dat U dagelijks oefent.

SERIE I. *(oefening 1 en 3 vervallen)*

Oefening 1 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. Stel U voor, dat U in Uw eigen huiskamer bent.
3. Kijk in gedachten in de kamer rond en neem bewust waar wat U "ziet".
4. Probeer zo levendig mogelijk te zien, wat U zou zien, wanneer U werkelijk in de kamer was.
5. Let ook op de details, zoals asbakken, planten, tafelkleedjes.
6. Doe de ogen weer open.

Oefening 2. (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich weer voor dat U in Uw eigen huiskamer bent.
3. U staat in gedachten op en wandelt richting buitendeur.
4. U neemt tijdens deze wandeling zo veel mogelijk waar wat U op Uw weg tegenkomt. U doet over de wandeling even lang als hij in werkelijkheid zou duren.
5. Wanneer U bij de buitendeur aangekomen bent, gaat U naar buiten en loopt tot aan de straat.
6. Vervolgens draait U zich om en wandelt dezelfde weg weer terug in gedachten, tot U weer terug bent in de stoel van waaruit U de wandeling begonnen bent.
7. U opent de ogen weer.

Oefening 3 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich voor dat U in een straat staat waar U vaak doorloopt, bijvoorbeeld een straat die op de weg naar Uw werk ligt of op de weg naar de supermarkt waar U vaak inkopen doet.
3. Begin in gedachten te wandelen door deze straat.
4. Probeer alle dingen die in de straat te zien zijn zo duidelijk mogelijk in gedachten waar te nemen.
5. Wandel langzaam in gedachten, zodat U alles goed in U op kunt nemen.
6. U opent de ogen weer.

Oefening 4 (duur: 5 minuten).

1. Ga in een stoel zitten. Sluit Uw ogen.
2. U stelt zich de straat uit oefening 3 voor.
3. Evenals in oefening 3 begint U weer te wandelen.
4. Nu blijft U echter bij een van te voren gekozen gebouw staan.
5. U bekijkt het gebouw in gedachten en probeert er zoveel mogelijk details van waar te nemen.
6. U opent Uw ogen weer.

SERIE II. *(oefening 1 en 3 vervallen)*

Oefening 1 (duur: 6 minuten)

1. Neem een voorwerp. Plaats het voor U op tafel en bekijk het gedurende ongeveer 1 minuut. Concentreer U op het voorwerp en probeer aan niets anders te denken.
2. Sluit Uw ogen en tracht het voorwerp het voorwerp U voor de geest te halen. (1 minuut)

3. Open de ogen en bekijk het voorwerp weer. Houdt Uw hele aandacht bij het voorwerp. (1 minuut).
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open de ogen weer.

Oefening 2. (duur: 6 minuten)

1. Deze oefening is identiek aan oefening 1. Maar nu probeert U allerhande details aan het voorwerp waar te nemen in de eerste minuut.
2. U sluit de ogen en tracht het voorwerp inclusief de details zo goed mogelijk in Uw gedachten waar te nemen (1 minuut).
3. Open de ogen weer en controleer of U in de voorstelling de details waarop U gelet heeft ook gezien hebt. Tegelijkertijd zoekt U naar nieuwe details om te onthouden. (1 minuut).
4. Idem als onderdeel 2.
5. Idem als onderdeel 3.
6. Idem als onderdeel 2.
7. Open de ogen weer.

Oefening 3 (duur: 5 minuten)

1. U legt Uw horloge voor U op tafel neer.
2. Volg de secondewijzer aandachtig in zijn bewegingen. Laat u niet afleiden en probeer Uw gehele aandacht bij de bewegingen van de secondewijzer te houden, gedurende 1 minuut.
3. Houdt een minuut pauze.
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.

Oefening 4. (duur: 6,5 minuten)

1. U legt Uw horloge voor U op tafel neer.
2. Volg de secondewijzer aandachtig in zijn bewegingen. Laat U niet afleiden en probeer Uw aandacht geheel bij de bewegingen van de secondewijzer te houden, gedurende 1,5 minuut.
3. Houdt een minuut pauze.
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.

SERIE III. *(oefening 1 en 2 vervallen)*

Oefening 1. (duur: 7 minuten).

1. Plaats een vijftal kleine voorwerpen voor U op tafel. Bekijk ze gedurende 1 minuut.
2. Sluit de ogen en probeer ze U voor de geest te halen, alle vijf en zo gedetailleerd mogelijk. (1 minuut).
3. Open de ogen en controleer of U ze alle vijf onthouden heeft, of de details correct waren en probeer verdere details te onthouden (1 minuut).
4. Idem als 2.
5. Idem als 3.
6. Idem als 2.
7. Open Uw ogen en vergelijk Uw voorstelling met de werkelijkheid.

Oefening 2. (duur: 7 minuten)

Deze oefening is identiek aan oefening 1. Alleen doet U de oefening nu met 10 voorwerpen.

Oefening 3 (duur: 7 minuten)

Deze oefening is identiek aan oefening 1. Nu doet U de oefening echter met 15 voorwerpen.

Appendix L

Conditie: .....
Nummer: .....

SPANNINGSHOOFDPIJNONDERZOEK

Naam: ..... Geboortedatum: .....
Adres: .....
Telefoon: .....

Aanmelding dd.: .....
Eerste gesprek dd.: .... uur.
mee (1) testen en vragenlijsten: .....
(2) enveloppen: .. groot, .. klein
(3) hoofdpijndagboekkaarten: ..... stuks.

Ontvangen testen en vragenlijsten: ....., dd. .....
terug verwacht vóór dd.: .....
reminder gestuurd dd. : .....

Opgestuurd dd. ....., vragenlijsten en testen: .....
terug verwacht vóór dd.: .....
ontvangen: .....
reminder: .....

Therapeut: .....

Opgeroepen dd. ..... voor eerste behandeling op .....
Eventuele wijziging eerste afspraak: .....

Therapieuur	datum	tijd	evt. wijzigingen in afspraak
1			
2			
3			
4			
5			
6			
7			
8			

Appendix M THERAPEUT SELF-MONITORING FORMULIER

N.B.: DIRECT NA ELKE SESSIE INVULLEN S.V.P.!

Patiëntnummer: .....

Sessienummer: .....

Datum: .....

1. In welke mate heb je je afgelopen therapiesessie aan het draaiboek gehouden?

   O erg goed
   O goed
   O ruim voldoende
   O voldoende
   O matig
   O slecht
   O erg slecht

2. De tijdsindeling

   a. Begin therapiesessie: ..... uur
   b. Begin oefeningen: ..... uur
   c. Einde therapiesessie: ..... uur

Appendix N

EVALUATIEFORMULIER I
Therapeut-vorm

Patiëntnummer: .....

Datum: .....

NB 1: Dit formulier alléén na sessie 1 invullen!
NB 2: S.v.p. slechts één antwoord per vraag aankruisen.

1. Hoe vond patiënte het gespreksgedeelte (= de eerste helft) van dit uur gaan, denk je?

   0 erg goed
   0 goed
   0 ruim voldoende
   0 voldoende
   0 matig
   0 slecht
   0 erg slecht

2. Hoe vond patiënte de oefeningen gaan, denk je?

   0 erg goed
   0 goed
   0 ruim voldoende
   0 voldoende
   0 matig
   0 slecht
   0 erg slecht

3. Denk je dat de patiënte de therapie die zij krijgt ook aan een kennis van haar die dezelfde klacht heeft of krijgt zou aanraden, als die haar zou vragen wat ze er het beste tegen doen kan?

   0 natuurlijk
   0 waarschijnlijk wel
   0 misschien wel
   0 dat weet ik nog niet
   0 misschien niet
   0 waarschijnlijk niet
   0 nee

4. Hoe denk je dat het met de hoofdpijn van patiënte na afloop van de therapie zal zijn?

   0 helemaal weg
   0 bijna helemaal weg
   0 veel minder dan vóór de behandeling
   0 ongeveer de helft minder
   0 een beetje minder
   0 hetzelfde
   0 erger

Appendix O

EVALUATIEFORMULIER I
Patiënt-vorm

Patiëntnummer: .....

Datum: .....

S.v.p. slechts één antwoord per vraag aankruisen.

1. Hoe ging het gesprek dat U vóór de oefeningen met de therapeut(e) had?

   O erg goed
   O goed
   O ruim voldoende
   O voldoende
   O matig
   O slecht
   O erg slecht

2. Hoe gingen de oefeningen?

   O erg goed
   O goed
   O ruim voldoende
   O voldoende
   O matig
   O slecht
   O erg slecht

3. Als een kennis van U ook spanningshoofdpijn heeft of krijgt en aan U vraagt wat ze er het beste tegen doen kan: Zoudt U deze kennis dan de behandeling die Uzelf hier krijgt, aanraden?

   O natuurlijk
   O waarschijnlijk wel
   O misschien wel
   O dat weet ik nog niet
   O misschien niet
   O waarsschijnlijk niet
   O nee

4. Hóe denkt U dat het met Uw hoofdpijn na afloop van deze therapie zal zijn?

   O helemaal weg
   O bijna helemaal weg
   O veel minder dan vóór de behandeling
   O ongeveer de helft minder
   O een beetje minder
   O hetzelfde
   O erger

BEDANKT VOOR HET INVULLEN

Appendix P

EVALUATIEFORMULIER II
Therapeut-vorm

Patiëntnummer: .....

Datum: .....

NB 1: Dit formulier alléén na sessie 8 invullen!
NB 2: S.v.p. slechts één antwoord per vraag aankruisen

1. Hoe vond patiënte het gespreksgedeelte (= de eerste helft) van dit uur gaan, denk je?

   0 erg goed
   0 goed
   0 ruim voldoende
   0 voldoende
   0 matig
   0 slecht
   0 erg slecht

2. Hoe vond patiënte de oefeningen deze keer gaan, denk je?

   0 erg goed
   0 goed
   0 ruim voldoende
   0 voldoende
   0 matig
   0 slecht
   0 erg slecht

3. Denk je dat de patiënte de therapie die zij gekregen heeft ook aan een kennis van haar die dezelfde klacht heeft of krijgt zou aanraden, als die haar zou vragen wat ze er het beste tegen doen kan?

   0 natuurlijk
   0 waarschijnlijk wel
   0 misschien wel
   0 dat weet ik nog niet
   0 misschien niet
   0 waarschijnlijk niet
   0 nee

4. Hoe denk je dat het de laatste tijd met de hoofdpijn van patiënte is?

   0 helemaal weg
   0 bijna helemaal weg
   0 veel minder dan vóór de behandeling
   0 ongeveer de helft minder
   0 een beetje minder
   0 hetzelfde
   0 erger

5. Hoe vaak denk je dat patiënte per week gemiddeld oefent?

   ..... keer per week

Appendix Q

EVALUATIEFORMULIER II
Patiënt-vorm

Patiëntnummer: .....

Datum: .....

S.v.p. per vraag slechts één antwoord aankruisen.

1. Hoe ging het gesprek vóór de oefeningen deze laatste keer?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

2. Hoe gingen de oefeningen deze laatste keer?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

3. Als een kennis van U ook spanningshoofdpijn heeft of krijgt en aan U vraagt wat ze er het beste tegen doen kan: Zoudt U deze kennis dan de behandeling die Uzelf hier gekregen heeft, aanraden?

O natuurlijk
O waarschijnlijk wel
O misschien wel
O dat weet ik nog niet
O misschien niet
O waarschijnlijk niet
O nee

4. Hoe is het de laatste tijd met de hoofdpijn?

O helemaal weg
O bijna helemaal weg
O veel minder dan vóór de behandeling
O ongeveer de helft minder
O een beetje minder
O hetzelfde
O erger

5. Hoe vaak oefent U gemiddeld per week?

..... keer per week

BEDANKT VOOR HET INVULLEN

Appendix R EVALUATIEFORMULIER III
Patiënt-vorm

Patiëntnummer: .....

Datum: .....

S.v.p. slechts één antwoord per vraag aankruisen.

1. Hoe gingen de oefeningen thuis meestal in de afgelopen 6 weken?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

2. Hoe vaak deed U de oefeningen gemiddeld per week, in de afgelopen 6 weken?

..... keer per week

3. Als een kennis van U ook spanningshoofdpijn heeft of krijgt en aan U vraagt wat zij er het beste tegen doen kan: Zoudt U deze kennis dan de behandeling die Uzelf hier gekregen heeft, aanraden?

O natuurlijk
O waarschijnlijk wel
O misschien wel
O dat weet ik nog niet
O misschien niet
O waarschijnlijk niet
O nee

4. Hoe is het de laatste tijd met de hoofdpijn?

O helemaal weg
O bijna helemaal weg
O veel minder dan vóór de behandeling
O ongeveer de helft minder
O een beetje minder
O hetzelfde
O erger

5. Kwamen de tijden waarop de behandeling plaatsvond U goed uit?

O ja, altijd
O meestal wel
O soms wel, soms niet
O meestal niet
O nee

6. Hoe vond U de duur van de therapie?

O te lang
O goed
O te kort

7. Hebben de oefeningen een goede of slechte bijdrage geleverd aan de behandeling van Uw spanningshoofdpijn?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

8. Hebben de gesprekken met de therapeut(e) vóór de oefeningen een goede of een slechte bijdrage geleverd aan de behandeling van Uw spanningshoofdpijn?

O erg goed
O goed
O ruim voldoende
O voldoende
O matig
O slecht
O erg slecht

BEDANKT VOOR HET INVULLEN

Appendix S EVALUATIEFORMULIER: Therapeut

Naam: .....

Datum: .....

1. Hoe heb je je werkzaamheden hier ervaren?

O zeer positief
O positief
O neutraal
O negatief
O zeer negatief

2. Wat vond je het meest positieve aan je werkzaamheden als therapeut?

.....

.....

.....

.....

3. Wat vond je het minst positieve aan je werkzaamheden als therapeut?

.....

.....

.....

.....

4.a. Heb je iets gemist in de inwerkingsperiode?

O ja
O nee

b. Zo ja, wat?

.....

.....

.....

.....

5.a. Had je zelf al enige ervaring met één of beide technieken, met eraan verwante Yoga technieken?

O ja
O nee

b. Zo ja, welke? .....

c. Zo ja, hoeveel ervaring? .....

6. Geef een punt (van 1 tot 10) voor de volgende werkzaamheden, die je als studentassistent verricht hebt (hoe hoger het punt, des te positiever je ervaring van het betreffende onderdeel van je werkzaamheden toen je deze verrichtte).

-- de eerste therapie-dag ..... _____

-- het therapie doen zelf:

- in het algemeen ..... _____
- het eerste, gespreksgedeelte van elk uur (de NSC-bespreking, e.d.) ..... _____
- het geven van ontspanningsoefeningen
  - -- in het algemeen ..... _____
  - -- in de OO conditie ..... _____
  - -- in de OC conditie ..... _____
- het geven van concentratieoefeningen
  - -- in het algemeen ..... _____
  - -- in de CC conditie ..... _____
  - -- In de CO conditie ..... _____

-- het 's ochtends therapie geven: ..... _____

-- het 's middags therapie geven: ..... _____

-- het 's avonds therapie geven: ..... _____

-- het meer dan 4 therapieën op één dag geven: ..... _____

-- therapie geven met veel vrije uren ertussen op één dag: ..... _____

7. Wat vind je beter voor de volgende "generatie" therapeuten (omcirkel het antwoord dat jou het meest aanspreekt):

A. Ze krijgen een 'vuurdoop' van 3 à 4 therapieën achter elkaar op de eerste dag
B. Ze krijgen de eerste dagen maar telkens 1 of hoogstens 2 patiënten per dag, waarna de hoeveelheid opgevoerd wordt.

8.a. Had je méér patiënten in de CO en/of OC condities dan in de CC en/of OO condities?

O ja
O nee

b. Zo ja, vond je dat vervelend?

O ja
O nee

c. Moeten we door 'matching' of iets dergelijks, zorgen dat dit in een volgende fase van het onderzoek vermeden wordt, ten behoeve van de volgende generatie therapeuten, of denk je dat dat niet nodig is?

O wel nodig
O niet nodig

9. Wat vind je van de kwaliteit van de opzet en organisatie van het onderzoek?

.....

.....

.....

.....

.....

10. Heb je zelf enige suggesties m.b.t. de opzet van het onderzoek (houd hierbij rekening met je ervaringen met de patiënten):

.....

.....

.....

.....

.....

11. Hoe sta je t.o.v. de ethische aspekten van het werken met experimentele controle en wachtlijst groepen, zoals wij die gebruikt hebben? (Houd hierbij in je antwoord rekening met het feit dat de patiënten in de controle condities na de evaluatieperiode de mogelijkheid hebben alsnog de juiste therapie te krijgen):

.....

.....

.....

.....

.....

12. Heb je enkele suggesties m.b.t. de ethische aspekten van het onderzoek, die de wetenschappelijke kwaliteit van het onderzoek niet in gevaar brengen?:

.....

.....

.....

.....

.....

13. Heb je nog suggesties of opmerkingen die in het bovenstaande niet aan de orde zijn gekomen, dan hier graag vermelden:

.....

.....

.....

.....

.....

Appendix T: F-values for the analyses of variance investigating the effects of Treatment Given (2) and Therapist Bias (2) on the Therapist Self-Monitoring Form (= TSM) data

TSM	item	Treatment Given (TG)	Therapist Bias (TB)	GT x TB
1	1	2.590	2.420	0.006
1	2	2.906	0.320	0.508
1	3	0.265	0.671	4.721+
1	4	2.146	0.116	2.449
2	1	1.286	0.016	0.016
2	2	3.108	0.677	0.677
2	3	11.448+	0.123	0.123
2	4	3.170	0.050	0.050
3	1	0.594	0.594	0.037
3	2	0.240	0.240	0.778
3	3	1.388	1.939	0.011
3	4	0.671	6.037+	1.193
4	1	2.576	0.330	0.033
4	2	0.160	0.296	0.028
4	3	0.125	1.848	1.366
4	4	0.024	1.649	2.555
5	1	0.924	0.682	0.329
5	2	1.480	1.887	0.137
5	3	1.397	2.307	0.002
5	4	0.066	0.010	0.158
6	1	0.302	1.503	0.001
6	2	0.441	3.031	3.749
6	3	0.276	1.897	1.592
6	4	3.336	1.205	2.506
7	1	0.749	0.239	1.011
7	2	0.098	0.684	3.083
7	3	0.181	0.404	0.258
7	4	0.821	0.635	3.869
8	1	1.302	0.002	1.561
8	2	1.010	0.339	0.784
8	3	1.016	0.345	0.533
8	4	0.031	3.956	0.196

+Significant at $<.05$ level

Appendix U: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on patient expectation of improvement at post session 1

Source of variation	df	mean square	F	p
Main effects	2	1.22	0.868	.426
Treatment Given (TG)	1	2.21	1.575	.215
Therapist Bias (TB)	1	0.20	0.142	.707
TG x TB interaction	1	3.23	2.308	.135
Explained	3	1.89	1.348	.269
Residual	53	1.40		
Total	56+	1.43		

+58 Cases were processed, 1 case of which was missing

Appendix V: Analysis of variance for the effects of Treatment Given (2) x Therapist Bias (2) on patient therapy credibility ratings

Table 1: Post session 1 ratings

Source of variation	df	mean square	F	p
Main effects	2	1.54	0.943	.396
Treatment Given (TG)	1	1.68	1.029	.315
Therapist Bias (TB)	1	1.40	0.856	.359
TG x TB interaction	1	3.06	1.875	.177
Explained	3	2.05	1.253	.300
Residual	54	1.63		
Total	57	1.65		

Table 2: Post session 8 ratings

Source of variation	df	mean square	F	p
Main effects	2	2.97	1.414	.255
Treatment Given (TG)	1	1.90	0.904	.347
Therapist Bias (TB)	1	3.95	1.877	.178
TG x TB interaction	1	2.40	1.141	.292
Explained	3	2.78	1.323	.280
Residual	41	2.10		
Total	44	2.15		

Appendix W: Global patient ratings of improvement at follow-up

improvement	experiment I					experiment II				
	RR	RC	CC	CR	total	RR	RC	CC	CR	total
1 (= totally gone)	0	0	0	0	0	0	0	1	0	1
2 (= almost totally gone)	1	2	1	1	5	3	4	1	1	9
3 (= much less)	5	2	3	3	13	2	4	4	2	12
4 (= about half less)	2	3	3	1	9	1	1	1	2	5
5 (= a little less)	1	1	1	0	3	2	1	1	3	7
6 (= the same)	1	2	0	1	4	4	1	3	2	10
7 (= worse)	0	1	0	1	2	1	0	0	0	1
One half or more (= 1-4)	8	7	7	5	27	6	9	7	5	27
Less than one half (= 5-7)	2	4	1	2	9	7	2	4	5	18

Uitgangspunt van deze studie is het nogal onrustbarende feit dat, ondanks meer dan veertig jaar intensief onderzoek, in de psychotherapie geen grote vooruitgang geboekt is. Er bestaat een groot aantal verschillende psychotherapiesystemen, die alle beweren effectief te zijn voor de behandeling van neurotische stoornissen en het aantal groeit nog steeds.

De meerderheid van de in zwang zijnde psychotherapiesystemen heeft weinig of geen onderzoek geproduceerd ter staving van hun therapeutische claims. Een aantal ervan heeft wèl effectonderzoek uitgevoerd. Echter, dit onderzoek heeft weinig meer opgeleverd dan de conclusie dat alle therapieën over het geheel genomen in gelijke doch bescheiden mate effectief zijn en dat de specifieke therapeutische technieken blijkbaar weinig tot het verhogen van de therapeutische effectiviteit bijdragen.

Deze situatie vertoont frappante overeenkomsten met die waarin de geneeskunde omstreeks de eeuwwisseling verkeerde. Uit het literatuuroverzicht in hoofdstuk 2 blijkt dat de geneeskunde zich in de loop van de eerste helft van deze eeuw aan deze situatie ontworsteld heeft. Gecontroleerd onderzoek, dat via 'enkel-blind' onderzoek culmineerde in 'dubbel-blind' onderzoek, was de drijvende kracht hierachter. In enkel-blind onderzoek weet de patiënt niet of hij de experimentele of de controle behandeling krijgt. Hierdoor wordt de factor 'patiënt bias' onder controle gehouden. In dubbel-blind onderzoek weet ook de behandelaar niet welke patiënt de experimentele en welke patiënt de controle behandeling krijgt. Op die manier worden zowel patiënt bias als therapeut bias onder controle gehouden. In pharmacotherapie-onderzoek bereikte men dit door uiterlijk identieke pillen te gebruiken in de experimentele en de controle condities.

Een overzicht van het effectonderzoek in de psychothera-

pie in hoofdstuk 3 laat zien dat nog tot voor relatief korte tijd geleden psychotherapie-onderzoek op het niveau van ongecontroleerde klinische behandelingsrapportages bleef. Gecontroleerd onderzoek van het enkel-blind type is pas sinds de jaren zestig toegepast en dat vrijwel alleen in de gedragstherapie. Dubbel-blind onderzoek, dat in de pharmacotherapie nu als een soort 'conditio sine qua non' voor adequaat onderzoek geldt, acht(te) men in de psychotherapie onmogelijk. In de voorliggende studie wordt aangetoond dat dit een voorbarige conclusie is: Er is een alternatieve manier mogelijk om therapeut-blindheid (het cruciale onderdeel van dubbel-blind onderzoek) te bereiken, die ook toepasbaar is in psychotherapie-onderzoek.

In het empirische deel van deze studie (hoofdstuk 4 en 5) werd deze alternatieve methode om therapeut-blindheid te creëren toegepast: In beide experimenten werden twee therapieën gebruikt. Relaxatie therapie was de experimentele behandeling en concentratie therapie was de controle behandeling. De controle behandeling werd door de experimentator zodanig geconstrueerd dat ze qua procedure zo veel als mogelijk leek op de experimentele behandeling. Ze bevatte echter niet de theoretisch kritieke ingrediënten van de experimentele behandeling. De twee therapieën werden aan zowel patiënten als therapeuten gepresenteerd als bonafide therapieën van gelijke effectiviteit. Verder werden, om de invloed van therapeut bias te onderzoeken, beide therapieën uitgevoerd onder zowel positieve als negatieve therapeut bias. In beide experimenten was spanningshoofdpijn het doelsymptoom. Vrouwen van ongeveer 18 tot 45 jaar waren de patiënten. Experiment II was in wezen een meer verfijnde replicatie van experiment I.

De voornaamste bevindingen van dit onderzoek zijn: (a) dat dubbel-blind onderzoek in de psychotherapie mogelijk is, en (b) dat, zeker wanneer de therapeut degene is die de effectmeting verricht, het gebruik van het dubbel-blind design nodig is teneinde spurieuze bevindingen te vermijden.

Franz L. Wojciechowski werd geboren op 25 mei 1951 te Heerlen. Na het behalen van de akte van bekwaamheid als volledig bevoegd onderwijzer aan de Pedagogische Academie "Christus Magister" te Heerlen in 1972, begon hij de studie psychologie aan de Katholieke Universiteit Nijmegen. In 1978 legde hij cum laude het doctoraalexamen af, met als hoofdrichting klinische psychologie en als bijvakken: vergelijkende en fysiologische psychologie, psychiatrie en psychopathologie.

Van 1979 tot 1984 was hij werkzaam als wetenschappelijk medewerker op de vakgroep klinische psychologie van het Psychologisch Laboratorium van de Katholieke Universiteit Nijmegen. Hier heeft hij zich bezig gehouden met psychotherapie onderzoek, onderwijs in de psychotherapie en psychofarmacotherapie, en het verrichten van psychotherapieën. In dezelfde periode behaalde hij de registratie als klinisch psycholoog (1981), gedragstherapeut (1981) en psycholoog-psychotherapeut (1983).

Hij studeert verder culturele anthropologie aan de Katholieke Universiteit Nijmegen, waarin hij in 1980 het kandidaatsexamen behaalde. In de zomers van 1980-1983 verrichtte hij veldwerk onder de Indianen van Connecticut (USA) in het kader van zijn doctoraalstudie. Hij verwacht in het studiejaar 1984-1985 de doctoraalstudie culturele anthropologie af te sluiten.

STELLINGEN

I

Dubbel blind onderzoek in de psychotherapie is niet alleen wenselijk; het is ook mogelijk (dit proefschrift).

II

De teleurstellende resultaten van het effectonderzoek in de psychotherapie in de afgelopen veertig jaar zijn voor een belangrijk deel te wijten aan het niet onder controle houden van de factor 'therapeut bias' en het gebruiken van zeer reactieve effectmaten (dit proefschrift).

III

De placebo response in de farmacotherapie (beter worden na toediening van een inerte pil) is vaak beschouwd als een graadmeter voor de suggestibiliteit en "goedgelovigheid" van de patiënt. In wezen echter is het een adaptieve response. Een leertheoretische benadering van dit fenomeen is daarom zinvol.

Wojciechowski, F.L. Het "placebo"fenomeen in leertheoretisch perspectief. Gedragstherapeutisch Bulletin, 1981, 15(2), 15-33.

IV

Het onderzoek naar persoonlijkheidscorrelaten van de placeboresponse is merendeels beperkt gebleven tot het zoeken naar psychopathologische persoonlijkheidskenmerken. Dit heeft weinig tot niets opgeleverd. Juist omdat de placebo response in wezen adaptief van aard is, lijkt het zoeken naar persoonlijkheidskenmerken die indicatief zijn voor psychische gezondheid meer kans van slagen te hebben.

V

De praktijk van de psychotherapie is in hoge mate een kunst in plaats van een kunde, waarvoor een zeker talent nodig is. Evenals een jarenlange muziekopleiding geen goede musicus kan maken van iemand zonder muzikale aanleg, kan een nog zo gedegen psychotherapeutische opleiding geen goede psychotherapeut maken van iemand, die daarvoor de aanleg mist. Helaas is het vaststellen van de aanwezigheid van deze aanleg voor de psychotherapie tot nu toe zeer problematisch gebleken. Hierdoor is een

adequate selectie van aspirant psychotherapeuten nog niet mogelijk.

Zie ook: Reik, T. Listening with the third ear. New York: Grove Press, 1948, pagina 3.

VI

Vele wegen leiden naar het psychotherapeutische Rome, maar niet elke weg is geschikt voor elke patiënt; hetzelfde geldt voor de psychotherapeut.

VII

Voor een verantwoorde uitoefening van de klinisch psychologische praktijk is een gedegen kennis van de psychopathologie en de psychodiagnostiek onontbeerlijk. Een herwaardering hiervan in de opleiding tot klinisch psycholoog is derhalve dringend gewenst.

VIII

Het 'publicatie-turven' als graadmeter voor wetenschappelijke productiviteit en de ermee gepaard gaande publicatie'dwang' hebben een zeer nadelig neveneffect: de individuele onderzoeker heeft nog nauwelijks tijd om zich door de steeds groeiende publicatieberg heen te worstelen die op zijn eigen vakgebied verschijnt, laat staan door die van verwante vakgebieden. Het ware dan ook aan te bevelen om periodiek een algeheel publicatie moratorium af te kondigen voor de diverse wetenschapsgebieden, om zo een refractaire periode te creëren, die gebruikt kan worden om kennis te nemen van en zich te bezinnen op de in de afgelopen periode verschenen vakliteratuur.

IX

Om betrouwbare gegevens te verkrijgen is een adequate onderzoeksmethodologie noodzakelijk. Om relevante gegevens te verkrijgen is een goed idee, een zinnige vraagstelling nodig. Methodologie kan daarom nooit een substituut zijn voor ideeën en creativiteit. Dit geldt ook omgekeerd.

X

De "ontdekking" van Amerika door Columbus in 1492 is, althans vanuit het perspectief van de inheemse bevolking (de Indianen), de grootste ramp uit de wereldgeschiedenis geweest: deze leidde namelijk direct of indirect tot de dood van meer dan dertig miljoen Indianen in de eeuw volgend op 1492.

XI

Het afschilderen van de Indiaan als "bloeddorstige wilde" of als een soort sprookjesfiguur die in de categorie elfen, kabouters en Sinterklaas thuishoort, heeft er effectief toe bijgedragen de genocide op de Indianen te versluieren. Ook nu wordt mede daardoor de Indiaanse bevolking van met name Noord Amerika nog nauwelijks als "serieus" onderwerp van studie beschouwd in wetenschappelijke kringen.

XII

Volgens de Indianen hebben zij Columbus ontdekt, toen hij op weg naar Indië verdwaald was. (Bron: Chief Big Eagle van de Golden Hill Paugussett, persoonlijke mededeling).

XIII

De belangrijkste methode van dataverzameling in de antropologie, de participerende observatie, is in hoge mate kwetsbaar voor intentionele en onintentionele vertekening van de waargenomen werkelijkheid door de onderzoeker. Deze vertekening wordt niet alleen aangetroffen in de beruchte, respectievelijk beroemde bijdragen van Carlos Castaneda (de Don Juan boeken) en Margaret Mead ('Coming of age in Samoa').

Wojciechowski, F.L. De Cariben van Dominica in veranderend anthropologisch perspectief. Intern Rapport 82KL02. KUN.

XIV

Doordat samenlevingen die de antropoloog bestudeert vaak aan snelle en diepgaande veranderingen onderhevig zijn, is verificatie cq falsificatie van diens data vaak zeer moeilijk.

XV

In de geestelijke gezondheidszorg m.b.t. allochtonen wordt te weinig gebruik gemaakt van de verworvenheden van de culturele antropologie.

Pluk, P.W.M. & Wojciechowski, F.L. Vergelijkende en interculturele psychotherapie. De Psycholoog, 1984, 29, 323-336.

XVI

Jeszce Polska nie zginęła (Polski Hymn Narodowy).

XVII

Luuj va Heêle, kal toch plat! Och mit de kinger ... (Anonieme Heerlenaar).

Stellingen behorend bij het proefschrift van F.L. Wojciechowski:
'Double blind research in psychotherapy'.

SWETS & ZEITLINGER B.V.